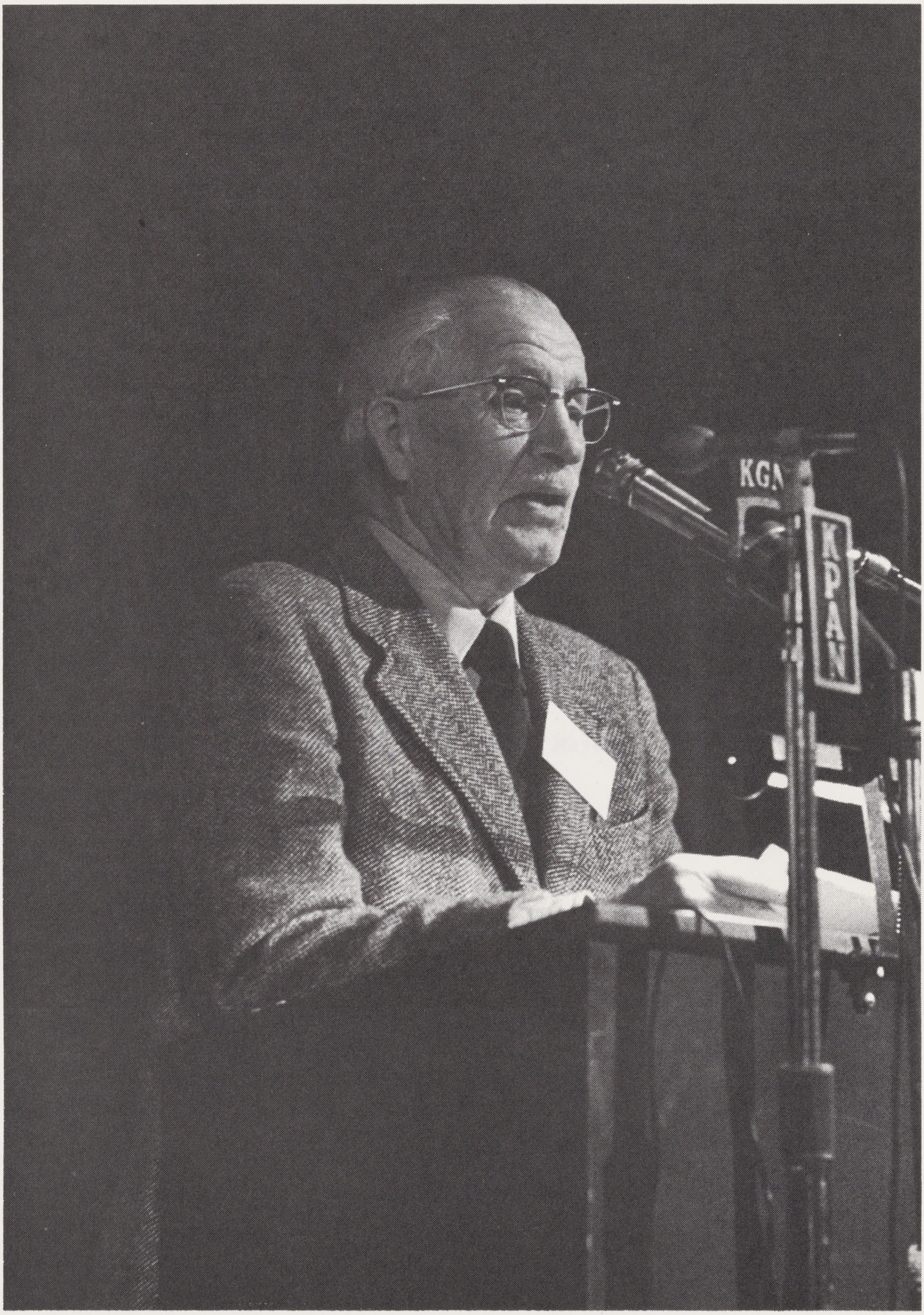

Speaking before the Texas Cattlemen's Association in Amarillo, Texas, Spring 1973.

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ADVENTURER IN ANIMAL SCIENCE:

HAROLD H. COLE

Introduction by James H. Meyer

Interviews conducted by
Reuben Albaugh, Irving I. Geschwind
and Hubert Heitman

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INTRODUCTION

Harold Cole, in brief, is a professors' professor and an ideal chairperson of an applied science department such as animal science. He can combine loyalty to the institution and loyalty to individual faculties in a constructive fashion. At times, the goals of the institution and the goals of the individual are in conflict, but Harold Cole was able to develop the correct objectives as a middle course.

His interest in, and ability to help the faculty develop appropriately, especially the young faculty, was important during his tenure as chairperson. He was able to instill an appropriate balance between basic and applied research so that all had a good understanding of husbandry and science of animal agriculture.

Harold Cole was an excellent teacher and researcher in his own right. Prior to my coming to Davis, I was aware of the many, many contributions that Harold had made to literature on physiology of reproduction in the thirties and the forties. After I came to Davis, it was quite apparent as one traveled throughout the state that most alumni from the animal husbandry program remembered Harold Cole's Animal Husbandry 110, Physiology of Animals, as a very important learning experience.

After retirement, Harold Cole continued an active research program working every day. He served willingly when asked to re-involve himself in the teaching program. It has been thirteen years since he has retired and it can be truthfully stated that he has been as productive during these thirteen years as many are in their first thirteen years as a faculty member.

James H. Meyer
Chancellor

HISTORY OF THE INTERVIEW

The importance of obtaining an oral history life memoir of Harold H. Cole was placed high on the priority list by the Oral History Center's Faculty Advisory Committee. Dr. Cole enjoyed an international reputation in his field of reproductive biology and also had played a major role in the history of his Department of Animal Science and in the larger history of the UC Davis campus.

However, as in all of the oral history projects, funding was the problem. Finally after several years had gone by without commencing the Cole memoir, former Chancellor Emil Mrak, a member of the Advisory Committee became impatient and sent a letter relating the problem to Carl Garrison, a great friend of the campus and of the animal science department. Garrison not only responded promptly with a generous check but pledged the support of other cattlemen whom he would contact. Only his untimely death prevented Garrison from carrying out his self-imposed assignment.

Department Chairman Eric Bradford also sent out letters to colleagues asking for their support of the oral history program. The response was immediate including a generous check from Harold Cole himself.

Not only were funds now available to cover transcribing, editing, indexing, binding and the many other related costs but several close friends and colleagues of Cole agreed to interview him in the course of the memoir.

Reuben Albaugh, animal extension specialist, who like Cole had grown up on a diversified livestock ranch questioned him on his growing up and college years. Several questions on his horse breaking and milking experiences drew colorful responses in particular.

Hubert Heitman, Jr., himself a former chairman of the Department of Animal Science was especially qualified to bring out the important aspects of Cole's own lengthy tenure as Chairman, and as a member of many campus committees.

Irving Geschwind, an authority himself in the field of reproductive biology and research collaborator with Cole resulting in the publication of a number of scientific papers, provided the inquiry into Cole's research motivation, his methods and findings. Geschwind also suggested the title used for the memoir.

James Meyer, Chancellor of UC Davis and former chairman of the department, who was recruited by Cole, wrote the preface to the memoir.

The Oral History Center provided the necessary supplies and equipment and aided in the topical editing. Lois Smith of the center did the transcribing and typed the final copy. She and the assistant head of the center, Kathy Butz, proofread the work. Cole himself did a meticulous job of editing his memoir.

The result is therefore a product of the combined efforts of many persons, all admirers of Harold H. Cole. His memoir is the third to be completed of animal scientists. The others are on Max Kleiber and James F. Wilson.

A. I. Dickman, Head
Oral History Center

* * * *

I met with Professor Cole about two weeks before we set a firm date for the interview. At that time we decided upon a short, general outline of the material we would cover. There was no great detail. Prior to the actual interview I thought about it several times in trying to outline in my mind my responsibilities. I tried to think of specific events to bring up, some humorous, and prepare for various lines of questioning to follow any possible responses.

Harold had gone to a great deal of preparation. He had researched some of the early history of our department and had prepared statements in this regard. In listening to the interview, it is obvious that this portion was read from his prepared notes. The interview went off easily with time as the only limit.

Hubert Heitman, Jr.
Professor, Animal Science

* * * *

Before interviewing Dr. Cole on his memoirs, I studied an outline prepared by Mr. Dickman. This outline contained questions regarding Dr. Cole's early life that he spent on a farm in Wisconsin, his schooling, and the interval of time while he was attending college.

Before interviewing Dr. Cole, we had a discussion regarding how this interview would be conducted. He was given a list of questions that I was going to ask. During the interview, other questions were developed to bring out the complete story of his life. At times during the interview, we stopped the tape recorder and discussed how we would proceed on the next part of the interview. This proved to be helpful.

Reuben Albaugh
Extension Animal Scientist, Emeritus

I was told that Professor Cole had already been interviewed by Reuben Albaugh and Professor Heitman, who had covered all aspects of his life other than his experience as a research scientist, and that was to be my assignment. Although I had known Harold for only the sixteen years I had been a member of the Department of Animal Science (I was the last faculty member he recruited as chairman of the department), we had collaborated in research on anti-gonadotropins during that period so that I had gotten to know him rather well. I also had for some time associated Harold's name with his earlier research on pregnant mare's serum gonadotropin, and I was well acquainted with his collaboration with Professors Hart and Goss over a long period of time in characterizing the biological and chemical properties of the hormone. Moreover, in the early years after joining the department, Jim Boda, who has since moved over to animal physiology, had told me of Harold's interest in bloat and milk fever in cattle. Nonetheless, to prepare for the interview, I studied the bibliography of his published papers which had appeared as an appendix to the tribute paid him by the Society for the Study of Reproduction when it awarded him the coveted Carl Hartman Medal (this was published in Biology of Reproduction along with the Carl Hartman Lecture which he gave on that occasion).

With all of that as background, with a store of anecdotes and remembrances which sixteen years of five-days-a-week-conversations had brought to light (many resulting from comparing our experiences, twenty years apart, as students of Professor Herbert M. Evans in the Institute of Experimental Biology in Berkeley), and with the usual stories one hears from fellow faculty members and former graduate students in the department, I felt that my preparation was adequate for my debut as an interviewer.

I was, therefore, surprised when, once we had set the date for the taping, Harold presented me with a written list of subjects about which I was to question him and the order in which the questions were to be asked! I found it humorous, but could rationalize it in terms of his desire to see that this honor accorded to him and the solemnity of the occasion, were not to be marred by the inadequacies of an inexperienced and inadequately prepared reviewer who certainly could never fully appreciate what Harold considered to be his important achievements in research: nothing must be omitted and nothing must be left to chance. Nevertheless, I decided that no interviewee (what a horrible word!) was going to make me a "straight" man and diminish the honor bestowed upon me when I was asked to interview him! I was going to get in those questions I thought important.

The taping session was scheduled to start at nine one morning and we were to use Harold's office for it. When we met that day, Harold thought that it might last five to six hours, so we planned to stop for lunch and then resume about an hour and a half later. (The estimate of the interview's duration was quite accurate.)

It was obvious that Harold had come well-prepared, for he had with him a sheaf of papers containing extensive hand-written notes. These were the cues and reminders he needed for the session, and each page was devoted to a different research area on the list he had given me. Throughout the session, he referred continually to these notes, to make certain that nothing he had considered important had not been covered. The taping was interrupted twice because of minor problems: once when the phone rang (there was no way he could stop incoming calls from reaching him), and once when the battery in the tape recorder began to weaken. Except for those two occasions and our lunch break, the session went very smoothly.

I tried at the beginning to get some background material, to find out what had attracted a farm boy from Wisconsin to a life as an academic animal scientist, but was quickly told that that had been covered in an earlier interview. I did manage, however, to stray from my assigned topic of his research accomplishments and the program he had outlined, to find out what had attracted him to Berkeley as a master's degree student and to have him recount, after some prompting, some of his experiences with Professor Evans; what was going on in that world famous Berkeley laboratory in the 1920's; why he had returned to the Midwest for his Ph.D. degree studies; what had attracted him to Davis; his remembrances of the Department of Animal Science he had found and which he later became instrumental in developing; and how he thought a Department of Animal Science had to develop in the future.

For me, the session was a stimulating one. If by some chance future generations of Davis students or faculty fail to read the typescript of that session, that would be their loss. I, at least, had had my curiosity satisfied, had learned much, and had enjoyed listening to that mainly unilateral diaglogue (which, I suppose, is what a good interview ought to be). I can only hope that the digressions didn't bother him too much.

Irving I. Geschwind
Professor, Animal Science

BIOGRAPHICAL SKETCH

Harold Harrison Cole was born February 11, 1897, at Marshall, Wisconsin and grew up on a diversified livestock farm in that area. He received a B.A. degree from the University of Wisconsin in 1920, an M.S. degree at the University of California, Berkeley in 1925, and a Ph.D. degree from the University of Minnesota in 1928. He was the recipient of an L.L.D. degree at the University of California in 1965.

Dr. Cole joined the faculty of the Animal Science Department in 1928 as Assistant Professor. He rose through the ranks and served as chairman of the Animal Science Department from 1952-1960.

Among his many achievements are the editing of several books including Introduction to Livestock Production. He also edited, along with Dr. Cupps, Reproduction in Domestic Animals which has had three revisions and Animal Agriculture with Dr. Ronning.

For his research achievements in animal physiology, Cole was named by his colleagues as the Faculty Research Lecturer at Davis in 1944, and in 1952 he received the F. B. Morrison Award, the highest honor of the American Society of Animal Science. He later was presented the Society's award in Animal Physiology and Endocrinology for his work. In 1956-57, he was a Fulbright scholar at the Royal Veterinary College in Copenhagen and was responsible, at Davis, for the initiation of the Livestock Advisory Committee to the Division of Agricultural Sciences.

In 1960 Cole received the Golden Fleece award from the California Wool Growers Association. He has served on numerous faculty committees and as chairman of the Davis Division of the Academic Senate and as University Marshal. His civic service has included a term on the Davis Planning Commission. He received the American Society of Animal Science Award in Animal Physiology in 1963 and the University of Minnesota Outstanding Alumni Award in 1968. He also received the Hartman Lectureship of the Society for the Study of Reproduction in 1974.

In 1969 he was honored by the University by Dedication of the Harold H. Cole Facility for the Study of Biology of Large Animals.

He became the first editor of the journal, Biology of Reproduction, in 1968.

EARLY YEARS WITH FAMILY

Albaugh: When and where were you born and raised?

Cole: I was born on February 11, 1897 on a 200-acre farm in Southern Wisconsin, about 25 miles east of Madison. This farm was just four miles northwest of Waterloo, Wisconsin and four miles northeast of Marshall, Wisconsin. Therefore, I have given different accounts of the place of my birth, depending upon mail addresses which vaccilated between the two places at the whims of the postal service.

While in high school my father purchased two additional adjoining farms to increase the acreage to 480. In the southeast corner of the homestead farm there was a ten-acre orchard established by my grandfather. During my period, however, it was not regularly taken care of and only occasionally had a full crop of fruit. I mention this because in the past few years this orchard has been converted into a subdivision. It lies about one-half mile from any road and therefore I would presume they had some serious problems with snow in the wintertime although if they put down a blacktop road the county will plow off the road for them. This then lies about three miles from the closest city limit and is an indication of the escape of people from the city to the country.

Another aspect of the farm that I want to mention at this time is that there was a spring which arose about in the center of the farm going southward and resulted in a marshy area in the southwest corner of the farm. I mention this for several reasons. First, the stream served as a playhouse for us as youngsters where we built dams; and secondly, my paternal grandmother was born in a log cabin within a hundred feet of the spring and my great grandfather from accounts from my grandmother washed his face in the spring each morning at least during the summer. Also, this was a campsite for Indians and my grandmother tells of the Indians camping in this area when she was a child. Evidence of this was indicated further by the fact that we picked up large numbers of Indian arrowheads and Indian hammers in this particular area.

Cole: Now regarding the houses on this farm, my grandmother continued to live in the big house (original home) with eight bedrooms, although during the winter she only occupied two rooms, a joint dining and kitchen room and an upstairs bedroom just above. During the summer at least one, and frequently two of her daughters and their families stayed with her.

My father's house was located about 100 feet from my grandmother's home. My grandmother's home was colonial in style whereas ours was midwestern.

Albaugh: Do you want to tell us the names of your grandparents, Harold?

Cole: My grandparents on my mother's side were Charles and Alice White. Her maiden name was Alice Pearsall and on my father's side, William Harrison Cole and Emily Emery Cole. My grandfather Cole died at about the time of my birth. My grandmother was the daughter of David Emery who took up this homestead farm in 1847, one year before Wisconsin became a state.

The close proximity of our homes meant my grandmother was very much a part of our family even though she lived separately.

Albaugh: What was her name, Harold?

Cole: Her name was Emily Emery Cole. Emily Emery was her maiden name. She was a very remarkable person in many ways. She had possession of the original home for as long as she lived even though my father had purchased the farm on which it stood when he was a young man. She also kept 100 White Leghorn chickens and had a large garden. As a small boy, I received two or three cents for helping her clean the hen house. She did not believe in spoiling children as you can see. Occasionally, I also did some hoeing in the garden for her but for the most part she did the housekeeping and took care of the chickens and garden. Seeing her in the garden with a burlap apron, one would presume she was destitute. However, she owned three farms in addition to the homestead which my father had purchased. In the summer, she would load up her buggy with fruit and vegetables and distribute them to needy people in the town of Waterloo. She could have well afforded to move to an apartment in the city and live a life of luxury. She had a tremendous drive, however, and she loved the simple country life. Another reason for her wishing to live in the thirteen-room house was to provide a place for her children to come to visit during the summer. Very often, two or three children with their families would be there at the same time. When they arrived, she let them take care of the house and she devoted her time to the chickens and the garden--and of course to the grandchildren.

Cole: My grandmother had great faith in God. When she prayed and asked for guidance it was with great fervor and confidence that her prayers would be answered; and I'm sure that in most instances they were because she did not ask for frivolous things. She also had great confidence in man. I'm sure that when she sold her farms, the buyers had little but their word as a down payment. She had such great confidence in them that they went to great means to make each payment on schedule. In her early eighties, failing eyesight forced her to move in with her youngest daughter in Milwaukee. Frustrated because she could no longer help others resulted in her death shortly thereafter. Not by her words but by her actions, she played a major role in influencing the direction of my life.

Now regarding the names of my parents and brothers--my father's name was Clarence Eugene Cole. My mother's name was Hattie White. My great-grandfather White migrated from England and I saw him on several occasions. He had a small garden which he kept immaculately clean of weeds and he always told us that if everyone kept their places as clean from weeds as he did there would be no problem from weeds at all. He lived until he was about ninety-five.

I had two brothers--an older brother, Dwight Eliot Cole, who became shell-shocked in France during World War I and eventually committed suicide. My younger brother, four and a half years younger than I, was Lawrence Charles Cole, and until about seven years ago, he operated the homestead farm. He had a very high-producing herd of pure-bred Holsteins with about fifty lactating cows. He also had Duroc Jersey pigs. Therefore, the homestead remained in the family for about 125 years.

Now I would like to say a word about my mother's and father's family for reasons which will become obvious. There were nine children in my mother's family who reached adulthood (one died as a child); and there were eight children in my father's family who reached adulthood and also one died as a child. My mother's brother, Fred, graduated in electrical engineering at the University of Wisconsin and my father's brother, Horace, graduated from the Divinity School at Yale University. I believe that excepting for one sister, who took up nursing, all the others in my father's family, at least went to normal school and obtained a teacher's credential. In my mother's family, she was the only one to go to normal school, although as I mentioned, her brother Fred graduated from the University of Wisconsin.

I mention my mother's and father's family because of the impact it had with my relationships with others. Our social life consisted almost wholly of contact with relatives. One of my father's sisters

Cole: married a high school principal and they regularly made their headquarters with my grandmother during the summer, although for several years Uncle William Knoelk attended summer school to obtain his master's degree.

Another of my father's sisters married a farmer who retired in Pasadena when I was still fairly young and frequently the family with five daughters also spent the summer with my grandmother. My uncle's last two daughers were born when he was about eighty years old. These two girls were at least sixteen years younger than the next youngest child.

My grandfather Cole was a highly respected farmer. He gave lectures at Farmers Institute in Wisconsin, a forerunner I guess of the Extension Service, with personages such as Professor Babcock, the inventor of the Babcock test for milk fat; Professor King, who invented a system of barn ventilation; and Dean Henry, who with Professor F. B. Morrison, was the author of the first edition of Feeds and Feeding. At least one of my grandfather's lectures on sheep production was published in the University of Wisconsin bulletin but unfortunately I have lost the copy of this bulletin.

Now next, a little bit more about my parents and brothers. My mother was a very highly disciplined person, an immaculate housekeeper and a wonderful cook; and a kind, loving mother in an austere sort of way. In our family, it just didn't seem appropriate to show affection but there was never any question in my mind but that the affection was there. She made many sacrifices for the family, especially for me, as I was a rather sickly child with some unknown disease of the eyelids as a youngster. I've often wondered in later years if the physician may have put me on a milk-free diet with the result that I had vitamin A deficiency. Later I was troubled by recurrent bouts of appendicitis. The cure was to deprive me of all food and give me one tablespoon of water once per hour. This problem was finally solved when I was a freshman at the University of Wisconsin. While attending a night service at the church I developed an excruciating pain. My father was contacted by my friend, Allen Burr, and he made arrangements for an appendectomy the next morning by Dr. Jackson in Madison. A laparotomy at this particular time was still a fairly hazardous procedure, but anyway, my recovery was uneventful and I was able to complete my fall courses.

Well, I'm getting away from discussing my mother. She had a good voice and spent considerable time teaching me to sing with very limited success. For many years she was president of the local womens club. I believe they called it the Dorcas Society. This club was concerned in studying both literature and political issues. She was famous for her bread and biscuit making and was frequently called upon to judge these items at the Dane County Fair in Madison.

Cole: My father was recognized as a good farmer but I felt that his heart was really in broader matters. He was the long-time chairman of York township and served as county supervisor for a considerable period of time. He was also on the Board of Directors of the local bank and on the Board of Directors of the Waterloo Canning Association. This canning association raised and canned peas and corn. When my younger brother and I took over the home farm he became manager for the canning factory's eight to ten farms. His judgement was sought on a wide variety of matters and only once did I know where he made a serious error in judgement. This related to the activity of Uncle Lewis White, who lived just across the road. He and my father had been close friends for twenty-five to thirty years or longer and exchanged work for anything that would require more man power and equipment than each one separately had, such as in filling silo, threshing grain, corn shredding, hauling lambs to market, and so forth. Well, my uncle's wife became ill with menopausal problems, I presume, and they had a hired girl to do the cooking and housework. My uncle became attracted to her, to this maid, and intimacies resulted. This greatly disturbed my aunt from Pasadena who was spending the summer with my grandmother. At about the same time my Uncle Horace Cole, a Baptist minister, arrived for a visit. Between the two, they decided the sinning should stop and unfortunately, they enlisted my father's help. My uncle, the sinner, was called in for a consultation and prayer. He violently resented this intrusion into matters which he considered to be highly private and bore a grudge against my father for the rest of his life. Not only should my father have stayed out of this confrontation but he should have attempted to discourage my uncle and aunt from this although my aunt was a very strong-minded person and there was probably little hope that she could be dissuaded.

Well, let's get back to more favorable aspects of my father's character and personality. In retrospect, I now believe that many of his actions as a farmer were directed primarily in encouraging his sons to be leaders in the livestock industry. For instance, he purchased two adjoining farms to increase the acreage to 480 acres which I previously mentioned. He raised purebred Percheron horses, purebred Holsteins, purebred Duroc Jersey swine and also had a grade Shropshire flock. He also fed out a carload of lambs each winter to utilize the pea vine silage which was the by-product of his fresh peas production. Usually he would raise about fifty acres of peas for this canning factory. This continued for about twenty years when aphis and other diseases forced us out of this endeavor. For a number of years we showed Percherons at Dane County Fair as well as at the state fair with moderate success. It was during the period when large importers of Percherons dominated both the state fairs and the International Livestock Exposition in Chicago. My father made contact with the Department of Animal Husbandry at the University of Wisconsin and for many years during my youth a student from the

Cole: university spent the summer working on our farm. Some of my closest friends resulted from this association.

Albaugh: Harold, I know that you worked on the farm a lot while you were young. Tell us about some of your activities, chores that you took care of while you were going to school and growing up on the farm.

Cole: Yes, well I worked on the farm in various capacities from the time I was a small child. It was just taken for granted that every morning we would help with the chores--milking cows and feeding the pigs before we harnessed up to drive the four miles to Waterloo to school. Well, this was during the high school period. Earlier than that, of course, I went to a one-room schoolhouse about two miles from my home, and we walked this distance for the most part winter and summer. During the summer, it was also considered that we were going to be working on the home farm which we did even during the Christmas vacations or any other vacation periods we just naturally went to work. As a result of this we had really very little time for activities, such as athletic activities and I did not participate in any athletic activities during high school or for that matter, during college. Also, our athletic activities were inhibited in other ways. That is, both my mother and father were extremely religious and they did not condone drinking, smoking, playing cards, going swimming on Sunday or playing ball on Sunday.

Albaugh: How about dancing?

Cole: No. Dancing was also verboten until I went to college. This was a great disappointment to me because I would have enjoyed it greatly as I later did. Nonetheless, I never did feel that I was in any way discriminated against. In other words our family home life was an extremely happy one and I have always been most appreciative that I learned to work early in my life.

Albaugh: What denomination were your parents religious-wise?

Cole: We went to the Methodist church. We always went to Sunday school followed by a church service and sometimes we'd go to night service as well.

Albaugh: Was your father a deacon or . . .

Cole: Yes. For about twenty or twenty-five years he was the leader of the Sunday school. I'm not certain what the capacity, what the name was, but anyhow, he was in charge of the Sunday school operation and for some period of time when I was in high school I taught some of the smaller primary grades as well.

Cole: I had always assumed that my father never drank but in 1936 I received a surprise. At that time I was on sabbatical leave in Washington, D.C. My father and an uncle had toured the southern states by bus and returned via Washington to visit us. My father had a very bad cold and was coughing a great deal. Finally, my uncle said he thought my father needed a hot toddy. My wife, Jessie, said, "Harold, we don't have any whiskey on hand but why don't you go out and get some?" I did and fixed a hot toddy for both my father and uncle--in one instance to cure the cold and in the other to prevent him from getting one. Just to be on the safe side, I fixed a bourbon and water for Jess and myself. This is the only occasion on which I knew of my father taking an alcoholic drink and I'm not certain if it was purely for medicinal purposes.

Albaugh: You spoke about showing Percheron horses and raising them. Were you interested in horses, Harold. Did you like 'em? Did you break your own horses?

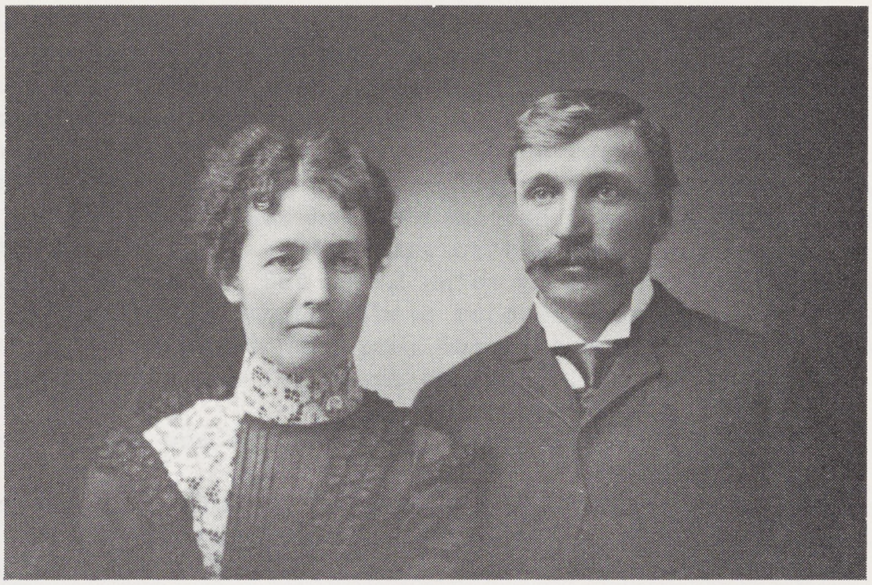
Cole: Oh yes, we did all our own breaking. This was an annual event in the spring to break any horses that reached the age of three years old and it was a rather simple procedure as we did it. We would take a steady horse, and harness this horse together with the horse to be broken, hook them onto a sled usually because of the fact that the sled could be turned at almost right angles without tipping over and this was when there would usually be some snow on the ground although there sometimes was no snow. We'd tie the new horse back to the hames of the broken horse and then just get in and start driving around. In a matter of a week or so we usually had this horse ready for spring's work although, of course it was still pretty green. We boys were actively concerned, of course, with the showing of horses and spent a lot of time during the summer evenings training the horses to stand, trot, and walk properly. We were guided in this activity by students from the university who were on the farm with us who had taken course work and had some experience along this line.

Albaugh: I guess I'll have to have you help us put on some showmanship demonstrations with our horse people.

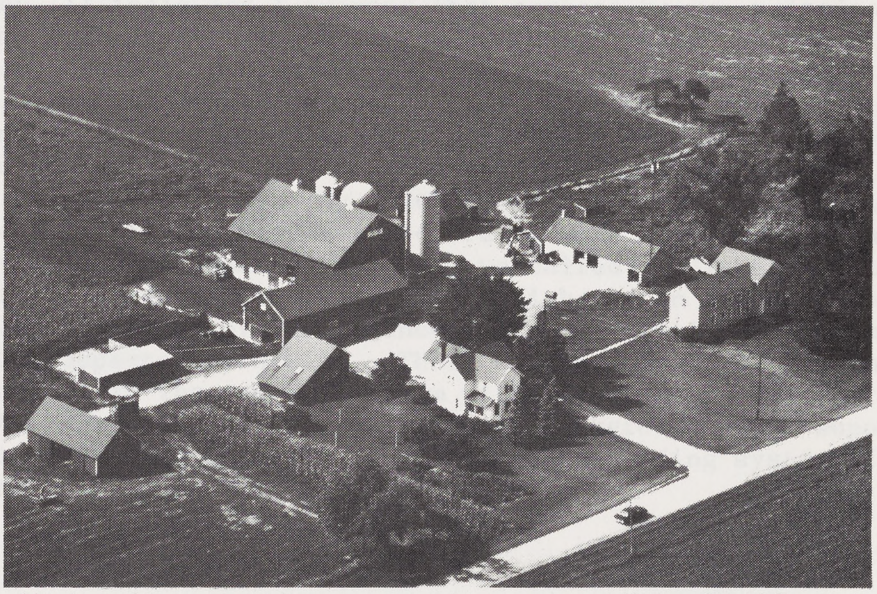
Cole: (Laughter) No, I'm afraid that I wouldn't be of much use now. It's been a long, long time, and of course the showing and halter is no different primarily between Thoroughbreds and for that of Percherons. There are certain fundamental rules that are applicable to both.

Albaugh: You spoke about your dairy cows. Were you considered a good milker?

Cole: Yes. I think that I could milk as fast as any of the people that we had. We always had one hired man, usually two hired men and



Clarence and Hattie Cole as young couple in Waterloo, Wisconsin, c. 1900.



Air view of homestead farm purchased by great grandfather David Emery in about 1845. House at left is my original home; at right is my grandmother Cole's home.

Cole: sometimes in the summer, three and I felt that I was quite a competent milker. As a matter of fact, when I first got out of college I did some advance registry testing of some purebred Holsteins. At that particular time, four times a day milking was in vogue and I was responsible for this activity. It was a rather tedious procedure. You met yourself getting up everytime you went to bed milking four times a day. Of course, by the time you finished milking the cows, feeding the cows, and taking care of the cows there was very little time left for sleep. But I did enjoy it and had fair success as far as records were concerned. These records were only, as I recall, for two or three weeks duration; but that meant that we milked the cows four times a day for about one month.

Albaugh: What encouraged you to go to high school?

Cole: I never really needed any encouragement anywhere along the line; that is this encouragement was given to me in indirect ways, I presume.

Albaugh: From your father and mother?

Cole: This was prompted by two things. First, they assumed from the time when I was a relatively small child, that I was going to go to the university and I presume that they were prompted some by my uncle, William Knoelk, who was principal of a high school. Incidentally, before he became Assistant Superintendent of Schools in the city of Milwaukee, he was principal of the high school in Waukegan, Illinois and I suppose that perhaps he was principal at the time that Jack Benny was in high school there. Although I don't ever recall of him (chuckle) talking about Jack Benny. Anyhow, they encouraged my folks to send us to college but I don't think my folks needed any encouragement because both of them had gone to normal school and they assumed that a certain amount of education was necessary. My older brother had gone to Lawrence College at Appleton, Wisconsin before he went into the army and my younger brother graduated from what we called the Intermediate Program at the University of Wisconsin, which was an agricultural program devoid of much of the sciences but specializing in the more practical aspects of farming. These students in their Intermediate Program were in the same classroom with the regular four year students but they did not have the science background and I suppose some consideration was given to that in the grading system of the students.

HIGH SCHOOL YEARS

Albaugh: What teacher had the most influence on your life in high school?

Cole: Well, I would say that there were several teachers in high school that had very important influence. I had some very outstanding teachers, I think, although I realized when I got to the university that my training was very inadequate. This was more because of the lack of courses, than it was of the teachers. There's Miss Rickerman who was my mathematic's teacher but we only had one year algebra and one-half year of geometry. We had no physics in high school. There were no courses available and the principal of the high school decided he was going to teach an advanced course in algebra the second year in algebra but it turned out to be a miserable failure and after a few weeks he stopped the course. Then there was Miss Emma Janisch, who was in charge of the teaching of penmanship and she was a very strict disciplinarian who had, I am sure, an influence on my life, although she did not teach me any penmanship. She very often said that she was very disappointed in me because my older brother had been very good at penmanship. Perhaps the most important influence was by a teacher who taught German, Miss Nettie Stokes, who was exceptionally proficient as a teacher of German and this was the only area when I got to college where I felt I was a little bit ahead of most of the students in the class and I think it went back to this excellent teacher. As a matter of fact we had a rather close relationship and she invited me on a number of occasions when I was in college to accompany her to the junior proms sort of thing, although I never did date her on a regular basis.

Albaugh: What about sports, Harold, when you were growing up? Did you hunt and fish, do any of that type of recreation?

Cole: No. There were practically no sports activities in our lives. As a matter of fact, we didn't even go swimming. Swimming on Sunday was verboten and consequently, the only time we could go swimming was in the evening. We did do this once during the summer at a mill pond which was about four miles away or, on very rare occasions, Cole: we went to Lake Mills where there was a rather large lake but not fishing. When I went to college I still hadn't learned to swim and in the fall of that semester, as I mentioned earlier I had an appendectomy and consequently my first semester in swimming was terminated abruptly for the rest of the semester, so I took swimming as my main physical education activity although in my junior year I did take up wrestling. I never did try out for the wrestling team but I was very much interested in it.

Albaugh: How about girl friends in high school?

Cole: No, girl friends in high school were practically nonexistent. Of course, at this particular time there were dances regularly. As I mentioned, my folks were opposed to dancing and I abided by their wishes until I got to college. The only other social activity--two other social activities--well, three perhaps; one would be we would go to Epworth League, that is a church activity, another one would be to go to ice cream socials, and I did date on two or three occasions during high school taking girls in a surrey with the hired man, who was a student at the university, to the ice cream socials. The other activity were box socials where the girls would fix up a box of dainty foods, cakes, etc. and these boxes would be auctioned off to the highest bidder and of course if we got the word that some particular person was enamored of a particular girl we would make him pay high for the box of the girl friend.

Albaugh: Who were some of your close friends in high school that had some influence on what you did?

Cole: Actually, I wouldn't say that any of my associates in high school had a great impact with what I did with the possible exception of Allen Burr. Allen was the son of the manager of the local canning factory for canned peas and corn and he and I became close friends in high school and remained close friends throughout our lives. He died a few years ago in northeastern Oregon. Also, when we got to college, we roomed at the same house and in the second year we were roommates at college. He was a very excellent long distance runner and he became interested in this because of his activities of having a trap line. He hunted skunks, muskrats and other animals for their furs in the winter and would run while he was checking his traps before and after school. He became a star long distance runner at the University of Wisconsin and I used to accompany him on many long jaunts one of which we took on one Thanksgiving afternoon for example. We walked and ran around Lake Mendota which is a distance of about twenty-seven miles. However, I don't think that so far as any of my activities were concerned, that he had any great influence on them.

UNDERGRADUATE YEARS AT UNIVERSITY OF WISCONSIN

Albaugh: Why did you go to college, Harold?

Cole: Well, this again, as I mentioned regarding school, that I had decided very early because of the influence of my father and because of the influence of my uncle, William Knoelk, the principal, that this was the thing to do. It never occurred to me to do otherwise. I was a very good student in high school and therefore, I felt that I was rather fairly well equipped. When I got to the university, however, I found that this was not true. I had taken no high school chemistry and very little mathematics and consequently this first semester in chemistry was really a rough proposition, as was the first course in physics because I had had no physics and most of the people that I was competing with, of course, had had considerable training in larger high schools such as Madison and Milwaukee. So I just more or less fell into the idea that I was going to go to college. Of course my ambition in going to college was to become a successful livestock producer, particularly raising purebred Percherons but as time went on and the tractor came into use in the 1915 to '20s I recognized that the handwriting was on the wall as far as draft horses were concerned and my emphasis changed from purebred Percherons to Holstein cattle. I was also interested in becoming a livestock judge and I was on both the dairy judging team and on the livestock judging team that competed at the National Dairy Show and the International Livestock Exposition when I was a student at the University of Wisconsin, and incidentally, was the high member of each team. I hoped to carry on this judging after I completed college and I did judge some county fairs at Wisconsin and started doing the same activity when I arrived at the University of California in Davis. However, I think it was at about the second or the third fair that I judged I ran into some extremely difficult classes in which there was some very strong competition of some of the best breeders in California as well as Arizona. I can't remember the name of the man that later went into the extension service who was at that time manager of the herd of Rosemary Farms. He was situated in Berkeley and was the dairy specialist for many years.

Albaugh: Was that [D. T.] Batchelder?

Cole: Yes! Batchelder. Rosemary Farms was one of the exhibitors at this fair in Santa Maria. Well, by the time I got through looking over this very difficult class of young heifers they were so near alike that the only way I could hope to make any judgment as to which one should win would be to look at their udders. By the time I spent time looking over the udders of from twelve to fourteen heifers the seat of my pants gave way. Well, I think that the person in charge of the livestock expositions decided that if a judge did not have enough money to have a decent pair of pants that would stand the wear and tear of judging it meant that he no longer belonged there and thus I think this was the end of my judging activity as a professional.

Albaugh: What happened when your pants gave way? Did you keep on judging?

Cole: Well, I don't recall--I think I--yes, I went on and finished the class. That was the last class of the day. Perhaps I just wanted to determine that this was the last class of the day, but anyhow we finished the judging the next day and I was very fortunate in being able to find a local tailor to repair my pants.

Albaugh: Well now in college in your undergraduate studies, were you interested in physiology there or were you interested in just animal husbandry?

Cole: No. I had absolutely no special interest as an undergraduate in any of the biological sciences, or for that matter, in the physical sciences. I took the courses because they were required, with moderate success, but my main interest was in what *I* thought were the practical courses. As a matter of fact, I talked my advisor out of my taking a course in genetics because I couldn't see where that could possibly do me any good. Of course, I regretted that later and had to take a course in graduate school to make up for this deficiency when I was receiving my master's degree in animal husbandry.

Albaugh: As a future author, did you take journalism, did you take public speaking in any of these courses?

Cole: Yes. I did take public speaking. As a matter of fact, I was on the high school debating team. This was about the only extracurricular school activity that I became interested in. I also took a course in debating at the University of Wisconsin. As a matter of fact, our high school debating team was coached by one of the main debaters at the University of Wisconsin at that particular time.

Albaugh: Did you join Alpha Gamma Rho while you were at the University of Wisconsin?

Cole: Yes. I joined the Alpha Gamma Rho when I got out of the Navy. I joined the Navy in February of 1918--well somewhat earlier I think--December of '17 was actually taken into the service in February and remained there until the end of the war which was on November 11, 1918 when the armistice was signed. I was at the time on board the U.S.S. Kentucky, a battleship.

Albaugh: How long were you in the Navy?

Cole: Just less than a year.

Albaugh: I was asking about Alpha Gamma Rho.

Cole: Well, on my release from the Navy I was in contact with my friend, who I have previously mentioned, Allen Burr, who was a member of Alpha Gamma Rho at that time; and I was then rushed for membership in the house. I have greatly appreciated that relationship throughout my life because I actually lived at fraternity houses at four campuses, that is the University of Wisconsin, later the University of California at Berkeley, the University of California at Davis and at the University of Minnesota.

Albaugh: Did you have any offices in this organization?

Cole: Yes. I think my highest office was secretary, as I recall.

Albaugh: Did this organization, was it quite active on the campus? Did it have a high scholastic average?

Cole: Yes. In every institution where I was I would say that the scholastic averages of the members of the fraternity were higher than the general average of the student body. As a matter of fact, it also had very high moral standards with the possible exception of the University of California at Berkeley, where drinking became a rather serious problem in the house and the activities became so obnoxious that I finally moved out of the house with another graduate student there so that I could devote more attention to my studies.

Albaugh: Who was the faculty officer that impressed you at any of these Alpha Gamma Rho fraternities?

Cole: There was a Professor Wilson who was a bee specialist at the University of Wisconsin, who later became the national secretary or president, I'm not sure which of Alpha Gamma Rho; and when I was at the University of California, Professor Essig, the entomologist was the advisor. I don't recall who the advisor was when I was living in the house in Davis. I think--yes--in retrospect it was Professor Robbins, the great botanist.

Albaugh: How about sororities, Harold? Did you people ever mingle with the sorority gals?

Cole: Yes. There was a great deal of contact with the sororities. As a matter of fact, during the rushing period one of our duties was to go around to act as servers at their meals and to entertain the girls and this entertainment was somewhat of a problem to me but I finally squirmed out of it by saying that I would like to sing a song entitled "A Bumblebee Backed Up to Me and Pushed". I had two or three other titles but anyhow this got enough of a laugh so that I was not forced to sing.

Albaugh: How about girl friends? Did you go to the Kappa Kappa Gamma house?

Cole: Yes. On occasion we dated girls from the sororities. I think that perhaps most of my girl friends were outside of sororities but there were certain functions where it seemed the thing to do to invite a sorority girl and I did go with members of several sororities including the Kappas to special activities such as the senior prom or the junior prom.

Albaugh: Did you have any nicknames in college?

Cole: Yes. I had several nicknames. I don't think I had a nickname at the University of Wisconsin. When I arrived at the Berkeley campus, because I used to talk a great deal about the Navy they called me bucket-of-Cole which is still used to some extent when I meet some of my old friends from the Berkeley campus. When I was at the University of Minnesota, my nickname was King Cole.

Albaugh: How about honorary fraternities while you were in school?

Cole: No. I did not make any of the honor societies. As an undergraduate my grades were not that good. I was about a B student. I was more interested in other activities than I was in gaining a high scholarship at that particular period. I did have a straight A average when I was in graduate school at the University of Minnesota but apparently at that institution they did not consider your grades as a graduate student. I was later made a member of Alpha Zeta, an honorary member while here at Davis.

Albaugh: Who were some of your professors who had great influence over your life?

Cole: Well, at the University of Wisconsin I came in contact relatively early with Professor F. B. Morrison who taught Feeds and Feeding and I took a special research project with him comparing corn and oats as a feed for working horses. At that time they did all their farm work at the University of Wisconsin with horses and therefore

Cole: we had two relatively large groups, one which was placed on corn, and the other one on oats. This was in combination, of course, with alfalfa and clover hay. The main result of that particular experiment was we showed that the two grains were about equal as far as our particular studies showed, that is we didn't go on to the more sophisticated approaches that we would use at the present time, of course. It was a question of weighing the horses and determining how they reacted while working. Did they sweat more and this sort of thing.

Albaugh: Do you remember any advice that Professor Morrison gave you about your future life?

Cole: No. No. It was more his actions, than anything else, that had an influence; I recall particularly, for example, we had to weigh these horses, usually on Sunday morning; you would get up at around 6:30 A.M. to weigh them before they were fed. Of course his attention to details impressed me at this particular time.

The other professor at the University of Wisconsin was Professor Fuller who was in charge of horses and also was in charge of the student livestock judging team. He kept in close contact with me even after I got out of college and it was mainly his interest in horses that appealed to me at the time.

Albaugh: Did you have any health problems while you were in college?

Cole: No. My health greatly improved. As a matter-of-fact after this first incident of appendicitis my health during college was extremely good. I had no problem.

FARMING ON SHARES

Albaugh: After you finished college I understand you went back to the farm. Is that right?

Cole: Yes. I farmed for about three years from about 1920 to 1923.

Albaugh: Tell us something about your activities on the farm.

Cole: Well, I was in charge of the 480 acres although my younger brother was there part of the time; at first only during the summer because he was still in school but I was in charge of the operations on the 480 acres. We discontinued the showing of livestock at this time. We had three married hired men at this particular time, one on each of the adjoining farms that had been purchased and another that lived in a small rental house that we had right near our home.

There really was nothing very exciting excepting I think that we did do a good job of farming as far as the dairy operation was concerned and as far as the raising of hogs was concerned.

Albaugh: Did you make money?

Cole: Well, this is questionable. We did not keep books as the modern farmer would. It was a rather hit or miss proposition. As a matter of fact I was running the farm on shares. Yes, I made money but I'm sure that if I were to put this on an hourly basis it would probably be a pretty small wage compared to that which we payed our hired men. They used to get fifty dollars per month. They'd get a house. They'd get two hogs per year that they could butcher. They could have fifty chickens. They had all of the milk they could use and so much butter, I think it was a pound or two of butter per week, and so on. I'm sure that they were paid, when all those things were taken into account, they probably made more than I did. However, I did save enough to finance, for the most part, the first two years of graduate study.

LEAVING THE FARM

Albaugh: What tempted you to leave the farm and go back to school?

Cole: Well, I developed a very severe case of hay fever which resulted in a sinus infection which was a most unpleasant experience. It reached such proportions that I had trouble sleeping and I lost about thirty or forty pounds and I decided that I just had to make some change. Therefore, I decided to give up farming in Wisconsin. In the meantime my younger brother had graduated and he was then in a position to take over the farms but I decided that I'd move to California with the idea of getting a master's degree to learn something about operations of farming in California and then go back into the dairy business. As a matter of fact, when I obtained my master's degree I had an opportunity to operate a big dairy in Marin County, the Bear Valley Dairy, which at that time was owned by an ex-colonel in the army. But in the meantime I had become interested in research due to the influence of two people, Dr. George Hart and Dr. Herbert MacLean Evans in the Department of Anatomy. Dr. Hart at that time was serving as Acting Chairman of the Department of Veterinary Science while Professor Haring was on a sabbatical leave and I also did a research project with him as well as with Dr. Evans. The research problem with Dr. Hart was not very successful whereas my activities with Dr. Evans were more fruitful. However, I became a very close friend with Dr. Hart at this time. He used to invite me to his home with his lovely wife and I was guided a great deal; as a matter-of-fact, he really served as my father from this time on until a number of years after I came back to the University of California at Davis. There was no opportunity for me to take a Ph.D. in Animal Husbandry at the University of California, of course, because they did not offer the degree and therefore I was encouraged in conjunction with Professor William Regan, at the University of California at Davis, to go back to the University of Minnesota and due to Professor Regan's help I was able to get an assistantship back there in the department of dairy husbandry.

Well, upon receipt of my master's degree in June of 1925, as I mentioned, I had the opportunity to take this job as manager of a

Cole: dairy in Marin County or to go on with graduate work. Because of the fact that I had taken a special project with Dr. Evans, he, for some reason, was favorably impressed and offered me a job as a research assistant to work with him for a year on the estrous cycle in the dog. I accepted this offer in preference to managing the dairy because I had caught the research bug so to speak, and this first research project proved to be a very successful enterprise. I was not paid greatly. I received eighty dollars a month but during the last four months of that year he also gave me an automobile to use to go back and forth between the campus and the place where I kept the dogs at the Humane Society, about seven miles distant. Prior to this time I had made this seven mile trip every morning, getting up around 5:30 just so I could be out there by seven by streetcar. I, of course, made many interesting contacts during this particular year which I retained throughout my life. I had great trouble contacting Dr. Evans at first because of his many activities at this particular time. I found that at noon he had a habit of dashing out the back door of the anatomy department, running down to the grocery store and picking up some cheese and crackers and a bottle of milk. Therefore, I would stand at the back door and wait for him to appear and walk down and discuss matters that I was particularly interested in at the time. Some decisions had to be made very frequently on where to go next on our research program. Eventually he invited me to accompany him and to have lunch with him and therefore, for the remainder of this period, I did have lunch with him rather regularly to discuss our research program. In April of that year however, he went to Europe and did not come back until September, but he did give me a full-time technician to work with me in making histological slides. This of course was a tremendous job because we were serially sectioning the ovaries which meant several hundred slides on each ovary and we saved every fourth section. He gave me, as I say, the help of a full-time technician which was rather unusual for a graduate student to have any such amount of help but I greatly appreciated this. As a matter-of-fact this girl's name was Marjorie Sadler and in 1928 Dr. Evans had become divorced from his first wife and married this technician. When I got back to the University of California at Davis in 1928, I went to visit Dr. Evans in Berkeley. We had not completed the writing of the research effort on the estrous cycle in the dog. Well, one of the first things he brought up was he wanted to know what my feelings were regarding Marjorie Sadler. Well, I said that I thought she was an unusually fine person and he agreed with me enthusiastically. He did not tell me at the time but two weeks later he married her. There was one child born to this marriage but he was later divorced from this wife also. But anyhow, when he got back from Europe in September I had made substantial progress and the main events that we were interested in had all been fairly accurately worked out. The main problem was to obtain sufficient numbers of animals through each stage of the cycle so that the data would be statistically sound. Well, he was apparently very favorably

Cole: impressed and from September until the end of December when I left to go to the University of Minnesota he worked with me daily in going over the slides and helping in writing the manuscript. There were several things however, that were left hanging fire. We had not finished the revised manuscript and there were one or two items that we were particularly interested in determining. One was this question as to whether mating actually had an influence on ovulation and the other one was to make certain as to the time of ovulation. Well, this particular part of the program was completed in my first fall at Davis, in 1928. Dr. Hart allowed me to spend a few months in Berkeley to finish the writing of the manuscript in the early spring of 1929 and to complete our studies, assembling the illustrations and so forth.

Albaugh: When you were going to graduate school Harold, I know that you met a couple of important people I've heard you talk about like Emil Mrak and Clint Jackson. Do you want to discuss those two individuals?

Cole: Yes. I didn't really have any close contact with Clint Jackson. Of course he was a member of the Alpha Gamma Rho fraternity on the Berkeley campus and I had contact with him then. I had closer contact perhaps with his brother, Bud Jackson, at that time.

Albaugh: His brother?

Cole: His brother, but I roomed my first year in Berkeley, with the future Chancellor Mrak. We had a room on the third floor of the house where we were living. This house was heated entirely with a fireplace on the first floor. Although I came from a rather frigid climate I almost froze to death this first winter. While Emil would sit around in his shirt sleeves I would have to put on my overcoat when we were studying in this third floor room but anyhow, Emil and I became close friends and the friendship has continued throughout our lives.

Albaugh: What about finances in going through college, graduate school and so on. Did you have money of your own?

Cole: Well, during my undergraduate career I had a very loose arrangement with my father. As I mentioned, I spent every summer working on the home farm, and every Christmas vacation I went home to work. Whenever we had a few days vacation I went home to work and I was not paid a salary during this time but as a result of this my father paid my expenses at the university. They were not very great. My expenses during my first year at college amounted to $300 which wouldn't even pay for enrollment at the present time. While I was at the University of California my father partially financed my first year as a graduate student while working for my master's. I did do additional work however. I worked part-time in the Department

Cole: of Veterinary Science cleaning guinea pig cages and as I mentioned, I worked during the summer with the Department of Veterinary Science as a milker and during my second year I worked part-time as a cashier checking in the milkers at the Milk Producers Company in downtown Oakland. This was not a high paying job. I don't recall exactly what I received but I know it was about a third of that received by the people distributing milk.

Then when I went on to the University of Minnesota I was on a teaching assistantship or in my last year on a research assistantship so I only had to call on my father at limited times for extra money to finance some professional activity.

COMING TO UNIVERSITY OF CALIFORNIA, DAVIS

Albaugh: How did you happen to get the job here at the University, Harold?

Cole: Well, as I mentioned, Dr. Hart and I were very close friends and he really acted as a father practically. He was apparently impressed with the work I had done on the estrous cycle in the dog with Dr. Evans. He was also impressed I think, with my ability as a milker because the very first summer on the Berkeley campus the milker quit about two weeks before the end of school and he called me and asked me if I would be interested in taking over the job of milking the fifty head of cows that they had in the dairy located in Strawberry Canyon which was just above the Memorial Stadium. I said "yes" I would be glad to do that so I milked this string of cows during that first summer. Well, this kept me in close contact with Dr. Hart and because of the reports that he received from Evans he was favorably impressed and he told me before I left for Minnesota that he hoped that there would be a position available for me when I had completed my work at the University of Minnesota. In the fall of 1927 he was in the East attending a meeting of the American Veterinary Medical Association and he stopped by at St. Paul and talked with me and talked with my major professor, Professor C. H. Eckles, who said I was doing a very good job: "the only thing that I'm worried about is how I'm going to find a job for him when he completes his work"; because there were only one or two other physiologists in the Department of Animal or Dairy Husbandry in the United States at that time so it didn't look like the opportunities would be very extensive. Well, Dr. Hart told him that he was thinking of hiring me. In the meantime I had been contacted by Dr. Graves who was in charge of dairying at the U.S. Department of Agriculture in Beltsville, Maryland and he offered me a job. So when I had completed my work I had two opportunities; one at the U.S.D.A. at Beltsville and the other at the University of California at Davis. Well, although I was offered a better salary at Beltsville than at Davis I decided to accept the Davis position, first because of my close association with Dr. Hart and secondly because it would give me an opportunity to do teaching as well as research.

Albaugh: Well, what salary did you get?

Cole: I think the first year I was hired at $2600. After a year my salary was increased to $3000 which was rather unusual to receive an increase after the first year (perhaps I should not go into this) but the reason for this was that it was during my first year of research, which in cooperation with Dr. Hart was extremely successful, that we had discovered the gonadatropic hormone in the blood of pregnant mares.

HISTORY OF THE TEACHING OF LIVESTOCK PRODUCTION; UNIVERSITY OF CALIFORNIA AND ORIGINS OF THE UNIVERSITY FARM AT DAVIS

Heitman: Well, Harold, since we're going to talk for the next hour or two about your professional career in teaching and in the Department of Animal Science as an administrator, maybe you would like to start us off by saying a little bit about the background of animal science teaching in the University.

Cole: Yes, Hubert, before talking about my tenure at UCD, I would like to review briefly the early history of the teaching of livestock production prior to my arrival at the University of California. The first course relating to livestock production was taught on the Berkeley campus by Dr. Myer E. Jaffa, Assistant Professor of Animal Nutrition in 1896-97, the latter being the year of my birth. The course, given at Berkeley, is listed in the University of California Register (the equivalent of the University of California catalogue) as Animal Nutrition 3a, Analysis of forage plants, foods and food products. The following year this course is listed in the same manner but in the 1898-99 Register a course, Animal Nutrition 3a, Foods and Feeding is given by Professor Myer E. Jaffa and a Mr. Colby. This course is described as a lecture course "On cattle foods and their relation to nutrition, and on the principles of rational feeding of farm animals." This course later was given the more conventional title, Feeds and Feeding, and Professor Jaffa continued teaching it well after a similar course was offered on the Davis campus. Professor Jaffa's versatility is attested by the fact that not only did he teach this course in Feeds and Feeding but for many years also taught the only two courses offered in Poultry Husbandry, first at Berkeley and after 1908 in Davis, and in addition he gave a course on Chemistry of Dairying. Professor Jaffa's interest in the chemical composition of animal and human foods persisted throughout his tenure in the University; in 1924 when I arrived in the Berkeley campus, Mr. Harold Goss, Professor Jaffa's assistant, had as his main duty the chemical analysis of feeds and foods.

A new dimension in the teaching of livestock production was initiated in 1900-01. In that year, according to the Report of the

Cole: Secretary to the Board of Regents, Dr. Leroy Anderson is listed as instructor in dairying.

The 1900-1902 Biennial Report of the President [Benjamin Ide Wheeler] under the heading "Dairy Husbandry" states as follows, "The new Department of Dairy Husbandry has received one-tenth of the increase in the state's support of the University. This work was first organized by Professor Leroy Anderson. Dr. Anderson performed a most valuable service in planning the work of the Department, in lecturing at Farmer's Institutes, in visiting creameries and dairies to advise with the dairymen and to stimulate advances in dairy husbandry, in conducting correspondence courses, and in organizing a ten-week short course for practical dairymen. In the short course, students received instruction in methods of milk testing, in the handling of dairy products, and in the care of stock, and they were drilled through daily practice in the actual making of butter and cheese of a very superior quality.

"To the very great regret of the University Professor Anderson resigned to become director of the new Polytechnic School at San Luis Obispo. He was succeeded by Mr. E. W. Major of the University of Minnesota, a native of the Island of Jersey, a graduate of the University of Minnesota, and for a number of years engaged in its agricultural investigations. Mr. Major has made a beginning in the organization of the University herd."

This same report states that the students in agriculture represent 1/30th of all students in the University, but the Agricultural Department receives 1/8th of all income available for educational purposes.

It is clear from this report that the University administration had definite plans in initiating a full-scale program in the teaching of livestock production on the Berkeley campus.

The 1902-04 report of the President [Benjamin Ide Wheeler] points out that the total number of students in the University of California [Berkeley] was 3275 for 1902-03 and 3277 for 1903-04. Concerning students in agriculture it states, "During the past year the total number of students in agricultural classes, including the short courses, was 415, of whom 278 or 67 per cent, were from other colleges than that of agriculture, e.g. students not taking the full agricultural course. This fact alone shows how impossible it is to consider a removal of the college from Berkeley."

E. W. Major is now listed as Assistant Professor of Animal Industries and "The dairy farm has been equipped with an excellent herd."

Cole: The Biennial Report of the President in 1904-06 states that Professor Major is giving four courses: Animal Industries 18a - Cattle, sheep and swine; 18b - Horses; 18c - Livestock management 19 - Dairy husbandry.

Therefore, it is very clear, that although Dr. Leroy Anderson set up short courses in livestock production, Professor Major was the first to give conventional animal husbandry courses excepting for a course given by Professor Jaffa in the Department of Nutrition, Foods and Feeding. Professor Major gave the courses listed above through 1907-08 on the Berkeley campus and it may be that some animals other than dairy cattle were purchased and kept in the same area as the dairy herd. I have not found evidence to support this, however, and I can find no evidence as to where the dairy herd was housed. The 1908-09 Biennial Report of the President, page 36, states that land in Strawberry Canyon has been purchased at an expense of $162,000, and a total expense of $233,000 will be reached. This same report states, "The work in agriculture both of instruction and research is at present growing more rapidly than any other activity of the University." This Strawberry tract was no doubt to be used, in part at least, for livestock but where the dairy herd was housed in 1907-08 is unclear. Whether animals other than dairy cattle were actually purchased for use on the Berkeley campus is unknown but it is most likely that an act of the legislature reported in the 1904-06 Biennial Report of the President may have had an influence on developing herds of the various classes of livestock for the Berkeley campus, "The appropriation of $150,000 made by the legislature for the purchase and equipment of a University farm has permitted the purchase by the University Farm Commision of the Sparks, Wright and Hamel tracts of 779 acres at Davisville. The amount paid for this was $103,290. Plans have been drawn for two cottages, a stock-judging building, and a dairy building, and bids for construction have been received, amounting to $36,000."

This report also states, "Meantime, by the will of Mr. Theodore Kearney of Fresno, there has been given to the University an extraordinarily valuable tract of land of over 5400 acres in the neighborhood of Fresno."

The report states again, "The property [Kearney property] which thus goes into the possession of the University is yielding at present a net income of $57,000 to $60,000. This income will be sufficient for the maintenance of an experimental station and in some part for the maintenance of the school. The serious question is now presented to the Regents and the people of the state whether two state farms shall be maintained; and it is a question that ought not be considered from the point of view of local priorities but from the point of view of the interest of the whole state."

Cole: In discussing how many "agricultural high schools" should be maintained by the state this 1904-06 President's Report states, "The immediate problem with which we are now dealing is the creation of a university farm which can serve as a laboratory of farm practice in connection with the regular course in agriculture and can be, at the same time, used for experimental work, and as a site for a secondary school in agriculture." It is very obvious that President Wheeler was irritated by the unilateral decision of the legislature to set up a school at Davisville but he concludes, "The fact that the farm has been purchased by the commission, duly appointed by the legislature, seems to make it incumbent upon the University to go on with the institution at Davisville unless otherwise instructed by the legislature."

The teaching of livestock production courses on the Berkeley campus went on very much as I have indicated above for the years 1904-06 until 1908-09. In 1908-09 a dramatic change occurred in the teaching of livestock production courses; for the first time, most of these courses were given on the Davis campus. Professor Major was in primary charge of seven courses that year--two at Berkeley and five at Davis. Feeds and Feeding was given by Professor Jaffa on the Berkeley campus and Animal Industries 21, Feeding, care and management of dairy cattle by Professor Major on the Davis campus. Assisting Professor Major was Herbert A. Hopper, assistant professor of dairy husbandry. Obviously, there was still considerable uncertainty concerning the department or departments to be involved in the teaching of livestock production courses.

In 1909-10, Professor Major is still giving seven courses but now all but one are given at Davis.

Mr. A. M. Cleghorn is listed as principal of University Farm Schools. On January 1, 1908, Dr. Leroy Anderson accepted an invitation to return to the University of California as Professor of Agricultural Practices and Superintendent of University Farm Schools at Davis. These University Farm Schools were short courses ranging in length from two to eight weeks, each one usually being on a specific topic. Apparently Dr. Anderson did very little teaching to regular university students but in 1908-09 he is listed as teaching Dairy Industry II, milk and its products. Professor Major must have convinced his administrators that he was being over-worked because the following people are listed for 1910-11: Ernest W. Major, Professor of Animal Industries and Farm Manager, University Farm; Herbert Hopper, Assistant Professor of Dairy Husbandry; Howard Phillips, Instructor in Animal Industry; F. D. Hawk, Instructor in Animal Industry.

The courses given at Davis were: Animal Industry 100 - Breeding types of swine, horses and mules; Animal Industry 102 - Livestock

Cole: feeding; Animal Industry 103 - Principles of breeding; Animal Industry 105 - Livestock management; Animal Industry 106 - Conformation and soundness.

Professor Jaffa is still giving Feeds and Feeding on the Berkeley campus. I might add that Professor Jaffa gave the only two courses offered in Poultry Husbandry. In 1911-12, Leroy Anderson is still Professor of Agricultural Practices and Superintendent of University Farm Schools.

The only statement to appear in the Berkeley catalog under the title "Animal Industry" was the following, "Given only at University Farm. Special announcement to be made later." However, under Animal Industry 103, Principles of Breeding, is the statement, "Given at Davis and at Berkeley." This same course was given on both campuses the preceding year.

Teaching on the Davis campus in the earliest years was heralded by the appearance of circulars, The School of Agriculture on the University Farm, Davis, California. Circular No. 41 was the first of these which stated that instruction was to begin on January 5, 1909. Students should have completed the eighth grade of grammar school. Obviously this training referred to non-degree students. No list of courses was included.

Circular No. 43 describes the second year of instruction beginning on September 20, 1909. The first semester closed on December 18, 1909 and the second semester began on January 3, 1910 and ended May 6, 1910.

The non-degree courses are listed but the instructors are not. Circular No. 61 - University Farm School, Davis, California, Announcement for 1911-12, gives a list of areas of training in non-degree but no list of courses or instructors. The number of University Farm students (non-degree) are listed as follows: first class - 34; second class - 18; third class - 8; special students - 20.

The students for the preceding year are listed individually. It does list the college courses for the spring of 1912. College courses - University Farm, Davis are as follows: Animal Industry 3 - Breeds and types of dairy cattle and swine - Howard Phillips, B.S.A.; Animal Industry 102 - Livestock feeding - O. D. Baker, B.S.A.; Animal Industry 103 - Principles of breeding - James (sic) I. Thompson, B.S.A.; Animal Industry 105 - Livestock Management - O. D. Baker, B.S.A.

Circular No. 77 - University Farm School, Davis, California, Announcement for 1912-13, the instructors for degree, and probably non-degree, are the same as for the preceding year. The students for the preceding year are listed individually. Amongst those listed

Cole: are some names which are still familiar to many of us - Forest Fissel, John O. Rowe and George Wilson.

Inasmuch as the degree courses given at Davis are now listed in circulars, University Farm School, it is understandable why these courses are no longer listed in the Berkeley catalog.

For the spring semester of 1913, Frederick R. Marshall is listed as Professor of Animal Industries, Hubert E. Van Norman as Professor of Dairy Management, and John I. Thompson as Assistant Professor of Animal Industries. Apparently there was some uncertainty as to the naming of the department that was to become the Department of Animal Husbandry and later the Department of Animal Science because as indicated there were two professors of animal industry in the spring but in the fall of 1913 John I. Thompson is now listed as Assistant Professor of Animal Husbandry.

EARLY HISTORY OF THE DEPARTMENT OF ANIMAL SCIENCE AT DAVIS

Cole: Gordon H. True, Professor of Animal Husbandry joined the department in the fall of 1913 and apparently was responsible for changing the name from Animal Industry to Animal Husbandry. C. R. George is listed as Instructor in Animal Husbandry but apparently Frederick R. Marshall retained his title as Professor of Animal Industries. All of the courses in the fall of 1913 were listed as Animal Industry courses. The courses consisted of Types and breeds, Livestock management, and Feeds and feeding. Professor Marshall is listed as primarily responsible for all three courses with the assistance of Professor Thompson and Mr. George. That fall Professor True taught a course on Farm animals on the Berkeley campus. Apparently Professor Marshall left at the end of the fall semester as he is not listed in 1914.

In January to May, 1914, Professor True, Professor Thompson, Instructor George and assistants in Animal Husbandry, George Burge, V. F. Dolcinci and E. H. Voorhies all held Animal Husbandry titles. Therefore, it appears quite clear that the change in name from Animal Industry to Animal Husbandry is related to the coming of Professor True.

The arrival of Professor True, the addition of several faculty members, the shifting of courses in livestock production to the Davis campus and the change in name of the department from Animal industry to Animal Husbandry is the big announcement which had been heralded in the Berkeley catalogue. However, as I have pointed out the shift from Berkeley to Davis began in 1908.

In 1914 Robert F. Miller was appointed as Assistant Professor of Animal Husbandry, a title which he held until 1918 when he left to become Professor of Animal Husbandry at Texas A & M College. In 1920, he came back as Associate Professor of Animal Husbandry.

In 1914, E. H. Voorhies is listed as Assistant in Animal Husbandry. In 1916-17 he appears as Instructor in Animal Husbandry and was responsible for a course in milk production. He was also responsible for the dairy goat herd. Sometime between 1917 and 1928

Cole: the goat herd was dispersed. This was a very unfortunate move because, as has been subsequently demonstrated, there are many physiological problems relating to milk production which can be more economically attacked using goats as compared to cattle. In any instance, Mr. Voorhies was shortly moved to the Department of Agricultural Economics at Berkeley. He later became Dean of Students on the Berkeley campus but ended his distinguished academic career by teaching agricultural economics on the Davis campus.

Also in 1914, Dr. Fritz W. Woll was appointed as Professor of Nutrition on the Davis campus but he apparently was housed with the Department of Animal Husbandry. His relationship with the Department of Animal Husbandry is somewhat unclear though he did teach Feeds and Feeding for a time.

Obviously his major enterprise was the writing of textbooks. He was author, co-author or translator of the following texts: Woll, F. W. 1900. A book on silage; Woll, F. W. 1909. Edited 5th revision of J. D. Decker's Cheese Making; Woll, F. W. 1914. A handbook for farmers and dairymen, 6th ed.; Woll, F. W. 1908. Authorized American edition of the principles of modern dairy practice; Woll, F. W. 1921. Productive feeding of farm animals, 3rd ed. (Note: The 2nd edition appeared in 1916.); Farrington, E. H. and F. W. Woll. 1918. Testing milk and its products, 24th ed.; Woll, F. W. and G. H. True. 1925. Productive feeding of farm animals, 4th ed.

A memorial statement concerning Dr. F. W. Woll written by Professor G. H. True appears on page 11 of Vol. 11, No. 1 (Jan. 1923) of the California Countryman (formerly the University of California Journal of Agriculture). Professor Woll was born on May 23, 1865 in Bergen, Norway and received his Ph.D. in Agricultural Chemistry from the University of Wisconsin. He remained on the faculty at the University of Wisconsin until coming to the University of California in 1912. (This date is in conflict with the previous reference to his arriving in 1914). Professor True states, "Owing to the lack of equipment at Davis for the prosecution of fundamental research in animal nutrition for which he was trained, the work undertaken had to be centered to that of more immediate and popular appeal to the practical dairyman." His research consisted largely of feeding trials. He started a long time trial of the comparative value of alfalfa with and without grain and silage supplements as a continuing ration of dairy cows but this work was discontinued, "on account of stress of conditions imposed during the war."

Professor True stated that Dr. Woll played a leading role in Advanced Registry Testing of Dairy Cattle. Professor Woll died in 1922.

Cole: The memorial statement by Professor True clears up why Professor Woll, the only man in the Department of Animal Husbandry to hold a Ph.D. degree during this period made such a negligible contribution from the standpoint of research. Obviously most of his efforts were concerned with the writing of texts.

In the 1917-19 catalog the following additional personnel in Animal Husbandry are listed: R. P. Royce, Instructor in Animal Husbandry; C. V. Castle, Assistant in Animal Husbandry.

The following University Farm School [non-degree] courses are listed under Animal Husbandry: Animal Husbandry 01 - Stock judging; Animal Husbandry 02 - Stock judging; Animal Husbandry 05 - Stock management; Animal Husbandry 06 - Feeds and feeding. The instructors for these courses are not listed.

The Division of Animal Husbandry came into control of an experimental range in 1920. In an article, "Solving our range cattle problem", appearing in University of California Journal of Agriculture, vol. 6, No. 7 (Nov. 1920), Mr. George Wilson describes the leasing of the Barrett Brothers ranch near Shingle Springs. Mr. Wilson states that the "California Range Experiment Station had its inception some years ago in the thoughts of Professor Gordon H. True." In 1916, the legislature appropriated the funds for the specific use of the University of California Range Experiment but the final selection of the site was not made until the spring of 1920. An eight-year lease was signed with the option to buy the portion of land privately owned. The privately owned winter range of approximately 3000 acres was situated in the foothill range area at an elevation ranging from 900 to 1000 feet. The summer range was in the Eldorado National Forest, 30 miles from the winter range with an elevation ranging from 3000 to 6500 feet. This was a community range occupied by 3 other cattlemen. The late summer range (7000 to 9000 feet) had as its eastern border the summit of the high Sierras.

The entire herd of 455 Barrett Brothers' cattle of Shorthorn and Red Polled origin was purchased. The primary object of this enterprise was to determine the most efficient and profitable use of the foothill range area in combination with our level production valleys for the production of beef. The Division of Veterinary Science proposed to make a study of carcasses from animals sold from the ranch and Professor Kennedy in the Department of Agronomy at Berkeley agreed to make a study of plant species in the three areas.

Mr. George Wilson was appointed as manager. Unfortunately, the Animal Husbandry portion of the experiment was, in my view, poorly designed. In any case University administrators decided that research of this type was inappropriate to be carried on by the University and the lease was allowed to lapse. In my opinion a more appropriate

Cole: approach would have been to redesign the experiment. At any rate, the University decided later that range research was important. To continue range studies, an agreement was entered into between the U.S. Forestry Service and the University to allow the Division of Animal Husbandry to conduct experiments with cattle at the U.S.D.A. San Joaquin Experimental Range near Madera. This agreement lasted from 1935 to 1958. Mr. Kenneth Wagnon was placed in charge of the Animal Husbandry activities. Dr. George Hart, who became chairman of the Department of Animal Husbandry in 1926 and his associate, Professor Harold Guilbert guided our research there in cooperation with Mr. Wagnon. Some very important studies on the nutritive value of forages at different seasons and studies on supplementation of the range were carried out. Also, the Division of Zoology carried out some important researches in conjunction with Animal Husbandry on the competition between rodents and cattle in the utilization of range forage. The University and U.S. Forestry Service agreed that the relationship at San Joaquin Experimental Range was no longer mutually advantageous.

In 1960, the Forbes Ranch northeast of Marysville was purchased and a range committee was established to plan an interdisciplinary program of research. Later on, the holdings were extended and a new headquarters was established because the Forbes headquarters was jeopardized by the proposed dam to be built on the Yuba River. Fifty head of cattle which had been farmed out to Jim McDougal were brought to the ranch and feeder cattle were purchased to fully utilize the forage.

During the period that Professor True was chairman of the Department of Animal Husbandry, 1914-26, an outstanding record of winnings at livestock expositions was made by the department [for most if for not all of this period it was the Division, not the Department of Animal Husbandry and the Department of Agriculture in the University]. My first inclination was to speak of an "enviable" record but on this point there were mixed feelings in the academic community because at this stage the selection and fitting of livestock for livestock shows was an art and involved little if any science. Nonetheless, the records made drew attention to the fact that a branch of the College of Agriculture at Davis existed.

The major credit for the show-ring records belong to two herdsmen: Mr. Alec McDonald, who became beef herdsman in 1911 and Mr. George Philip who became sheep herdsman at UCD in 1914.

Most of the records of winnings at livestock expositions are recorded in the University of California Journal of Agriculture, a publication put out by agricultural students at Berkeley and at Davis. In vol. 1, No. 2 the winnings at the California State Fair in 1913 are recorded. At this show, "The livestock from the University Farm brought home forty-seven awards, including twenty firsts, Cole: ten championships and one grandchampionship. The latter was captured by Thickset Boy, a yearling Angus steer . . ." Championships were also won with swine, dairy cattle and sheep.

I would like to deviate at this point to mention two other items in the first issues of the Agricultural Journal. One is a biographical sketch in the first issue of Professor Hubert E. Van Norman who served as chief campus officer at Davis for a number of years. Although he held a professorship in dairy management I have found no indication that he actively taught. For at least three years, 1911-13, he was president of the National Dairy Show Association. In Vol. 9, No. 7 (November, 1923) an article appears by Dr. G. H. Hart on the World's Dairy Congress held in the U.S. Dr. Hart states further that Professor H. E. Van Norman of Davis was the President of the Congress and received an honorary degree from Syracuse University.

The other item is "A Ministers' Week at Davis", which is described in Vol. 1, No. 5, pp. 19-21. The original suggestion for such a week was made to President Wheeler by a clergyman. To sound out the feasibility of such a venture 1500 letters were sent to clergymen throughout the state. Twelve affirmative replies were received. In spite of this discouraging response "A program of eighty-seven events was scheduled for the five days of Ministers' Week to be held at University Farm, Davis from December 1-5 inclusive." "Dr. Warren H. Wilson, the authority on the rural church and country life said at Davis that, 'Both in attendance and in program Ministers' Week was the greatest occasion of its kind ever held in America.' Five hundred and one clergymen attended. The dormitory space did not suffice and thus eight sleeping cars were sidetracked at Davis and used as temporary hotels. One clergyman walked fifteen miles across the mountains to reach the nearest railroad and after attending a lecture on the value of purebred livestock, purchased a white Plymouth Rock rooster from the Poultry Department which he proposed carrying home, under his arm, for the improvement of his neighborhood."

Well, now to return to a discussion of livestock show winnings by the Division of Animal Husbandry, Vol. 3, No. 3, Nov. 1915 of the Agricultural Journal carries an article,"University Prize Cattle at the Exposition" by Professor Gordon H. True. "Concerning the University's exhibit the Breeders Gazette [the leading livestock journal] said that a surprisingly excellent exhibit of bullocks was presented by the University of California from its Agricultural Farm at Davis. Even those fairly well acquainted with the work now underway at the college in steer feeding were surprised at the number of prime and beautifully fitted bullocks which were sent into the arena from the Davis Farm." Professor True points out in this article that all steers shown were bred at the University.

Cole: Vol. II, No. 6 (March, 1915) has an article by Alec McDonald on "Thickset Boy." This steer was shown in three consecutive years by reducing the feed after each show season and then gradually bringing him back to prime condition for the next year. He was first shown at 9 months of age at a weight of 825 pounds and was champion Angus steer at the California State Fair. The following year (1913) he was again shown at the California State Fair at a weight of 1400 pounds and was made grand champion steer. In September 1914 he was shown for the third time at the State Fair and was again made grand champion. He now weighed 1750 pounds.

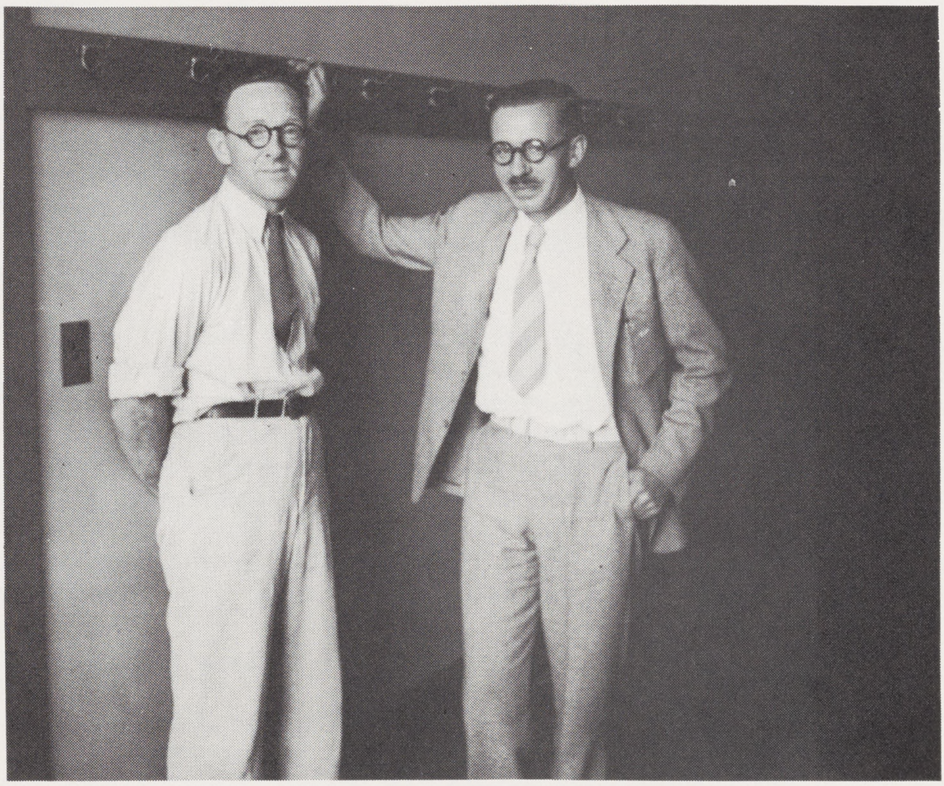
Vol. 3, No. 7 (April, 1916) of the Agricultural Journal carries an article written by Alec McDonald, "The Feeding of California King." This steer was made champion at the Panama Pacific Exposition in San Francisco and again at the Portland Livestock Exposition as a two-year old in 1915.

The showing of cattle at the International Livestock Exposition in Chicago continued through 1929, and perhaps longer, with spectacular success. Thus, showing was continued for several years after Dr. Hart's arrival as chairman.

During the period between 1916 and 1926 the following additions to the faculty in Animal Husbandry were made: 1919 - J. F. Wilson, Assistant Professor; 1920 - R. F. Miller, Associate Professor (returned from Texas A & M); 1920 - C. E. Howell, Assistant Professor, 1920-27, Associate Animal Husbandman and Animal Husbandman in Experiment Station, 1927-1950, Professor of Animal Husbandry in 1950 until retirement; 1920 - Elmer H. Hughes, Associate Professor, a title which he held for sixteen years before being promoted to Professor; 1922 - W. M. Regan, Associate Professor of Animal Husbandry, arrived from New Jersey with an inbred strain of Jerseys which was to occupy the major portion of his research time until retirement; 1924 - H. R. Guilbert, Junior Animal Husbandman in the Experiment Station, later he was given an academic title; 1924 - S. W. Mead, Assistant Professor but the following year he was deprived of his academic title; 1926 - George H. Hart, Professor of Animal Husbandry and chairman of the department from 1926-1948 at which time he relinquished this position to become Dean of the School of Veterinary Medicine (the UCD catalogue lists him as Professor of Animal Husbandry in 1925-26 but I know that he was on the Berkeley campus during that year). It is noteworthy that the list of faculty in the Department of Animal Husbandry is now complete until after World War II excepting for Dr. Harold Goss, Dr. Max Kleiber, Dr. Paul W. Gregory and myself.

On July 1, 1926 Professor True was relieved of the chairmanship and his only duties were the teaching of livestock production courses at Berkeley. According to some accounts, the administration feared that his continuing at Davis would interfere with the reorganization

Jessie Cliff and Harold Cole in 1925. Jessie visited me while I was in graduate school at Berkeley.



H. Goss and H. H. Cole, Animal Science Building, July 1937.

Cole: of the Division contemplated by Dr. Hart. Professor True had a particularly strong following amongst the purebred breeders. He also had a host of supporters in the membership of the American Society of Animal Production. In 1924 Professor True's portrait was hung in the Saddle and Sirloin Clubroom at the Stockyard Inn in Chicago. The American Society of Animal Production makes recommendations as to who is to be thus honored. In 1926, Professor True was the guest of honor at the annual dinner of the American Society of Animal Production in Chicago. Professor J. F. Wilson spoke in eulogy of Professor True.

One of the last issues of the California Countryman (Vol. 15, No. 1, September, 1928) has a memorial statement on Professor True who died June 8, 1928 at his Berkeley home.

Much of the information concerning the activities of the Division of Animal Husbandry between 1917 and 1928 has been gleaned from the University of California Journal of Agriculture (later the California Countryman). Though some of this information may appear in commercial livestock journals a great many articles are written by faculty members and not reported elsewhere. In my view this student journal performed a very useful function.

HIGHLIGHTS OF COLE'S EARLY CAREER AT DAVIS

Heitman: Now, Harold, this brings you up to approximately the time you came to Davis and did you come to Davis as a member of the faculty?

Cole: Yes, Hubert, I became a member of the Department of Animal Husbandry faculty in Davis in early October 1928 and excepting for sabbatical leaves in 1936 when I spent three months in Washington, D.C. and three months at Yale University and in Copenhagen, Denmark in 1956-57, I've been on the Davis campus continuously. The only other exception being a period of about two months in January and February 1929 when I was in Berkeley completing the writing of the manuscript on the estrous cycle of the dog with Dr. H. M. Evans.

Heitman: Listening to you tell about those early days raises, you know, interesting things in a person's mind when you go back to the time of Professor Jaffa, who was teaching a course in feeds and feeding. Today that would seem a practical type of course, I think, and the fact that Harold Goss came somewhat from that general direction and, of course, Professor Goss was always considered to be one of the very technical men on the staff at Davis but I think it points out that some of these technically trained people also have practical backgrounds.

Cole: This is very true. When I was doing my thesis work in Professor Jaffa's laboratory, that is the chemical analyses, in 1925, Dr. Harold Goss was determining the chemical constituent of a tremendously wide variety of foods and feeds that were sent to Professor Jaffa--everything from avocados to say alfalfa hay. Dr. Goss, then Mr. Goss, was exceedingly helpful to me.

Heitman: Those chemical analyses are still being used today for some things too because there is no more recent data available.

Cole: I presume that's true.

Heitman: You first came here on the faculty in October of 1928. Was that the first time you had come to Davis?

Cole: No, I had been here as a student in the fall of 1924 when I was getting started in my thesis work on calcium--the effect of calcium deficiency on reproduction in the dairy cow, under the auspices of Professor W. M. Regan. That fall, in addition to the thesis work, I did a special problem with Professor Gordon True on the early history of the purebred dairy industry in California. I wrote this report and it was to have been published in one of the livestock journals in California but because of a peculiar set of circumstances this never came about. As mentioned above, Professor True was shifted to the Berkeley campus. The appointment of Dr. Hart and the shifting of Professor True to Berkeley made Professor True quite bitter and understandably so. As a result of this I lost contact with Professor True and although I made some attempts with his children to recover the manuscript, which he had in his possession, I never was able to find it and perhaps it would have been destroyed before this time. Anyhow, this was an interesting sidelight and I regret very much that this history--early history--had been lost because of the fact that a great many of the details are no longer available. I was able to interview some of the people in the Palo Alto area where Senator Stanford had his purebred Holstein herd and I learned a great deal about the early introduction of cattle into California.

Heitman: Well, we're supposed to be putting some attention on your teaching assignments. When you came to Davis in 1928, what did you start teaching?

Cole: My first job when I arrived in Davis had nothing to do with teaching. The animal science building had just been completed and my first job was to help move the department from the old shingle building that still stands just north of Everson Hall (just across the street from Everson Hall to the north) to the new Animal Science building. This was not such a major undertaking as one might have supposed. All of the moving was done by the faculty and the staff in the Department of Animal Husbandry. There was no laboratory equipment. We didn't even own a test tube at that time, or a microscope, and therefore the moving consisted of moving the desks and the chairs, the books, and of course, the files. When I arrived, the fall semester which began in late August was well underway and Dr. Hart was teaching Animal Husbandry 110, The Physiology of Domestic Animals. Since he had taught part of the course, it was agreed that he should finish the lecturing in this course with the exception of a few lectures on reproduction which I gave and that I should spend my time helping in operating the laboratory, which I did.

Before I arrived, Dr. Hart had written to me about the equipping of the physiology laboratory. He'd also asked me about an elevator in the Department of Animal Science. I had written back and told him that I felt that for the moving of the equipment from the first to the second floor it was highly essential that an elevator

Cole: be installed. Well, lo and behold, when I got here there was an elevator, namely a dumbwaiter, which is still here but is now sealed off. As far as the equipment is concerned, there was nothing in the physiology laboratory or in the genetics laboratory for that matter, with the exception of some flat top tables with gas and electrical outlets.

Dr. Goss, on the other hand, was more fortunate in that he was in Berkeley and he had planned the laboratory to meet the special needs of a biochemist. His equipment arrived about the same time that I did and therefore, I did help him move in his equipment at that time.

At the time that I arrived, members of the department were (this is somewhat repetitive) George Hart, who was the chairman; James F. Wilson, wool technologist; Robert F. Miller, in charge of sheep production; Harold Guilbert, in charge of beef production; W. M. Regan, in charge of dairy production; S. W. Mead, dairy production; Arthur Folger, in charge of advanced registry testing; Elmer H. Hughes, swine production; and Carroll E. Howell, horse production; Dr. Harold Goss, animal nutritionist. And the following year Max Kleiber joined us from Switzerland to be in charge of metabolic studies and two years later Paul W. Gregory joined us as a geneticist from Harvard. Harold Guilbert and S. W. Mead held only experiment station titles for several years but they eventually did obtain professorial titles.

Heitman: Can we get back to old Animal Husbandry 110. When did you actually start in on that?

Cole: Yes. Well I did the laboratory work in the fall of 1928. In 1929 I had full charge of the course and remained in full charge until 1951 when I took over as chairman of the department and Professor Boda taught the course that year and of course, for several years thereafter.

Heitman: I always think that this is something worth mentioning because anyone who ever had that course was always impressed with the thought that it was a very well taught course. A case of an instructor who never came to class unprepared, a very demanding instructor and one from whom the students always learned a lot and I think it was a wonderful thing that you taught it for so many years and then a student of yours taught it and then I guess almost a student again taught it. I guess it went into--now it's being team taught.

Cole: Right.

Heitman: And even though it was called--it's now called Physiology 110, eventually went to 110A, B and is now 110A, B, C. I don't know that

Heitman: anyone who ever took it when it was called Animal Husbandry 110 would ever think that the course had changed that much. And you might be interested to know that in the past few years, I've had several of our old advisees contact me who have been interested in going back and teaching and everytime they go to the State Department of Education about getting a credential, the question comes up as to what these old animal husbandry courses were and could anything that was called animal husbandry possibly prepare a student to teach biology in a grade school or in a high school. I think the State Department of Education now realizes that some of our courses, particularly old Animal Husbandry 110 should have been called through all those years Physiology 110--it really is a biology course.

Cole: Yes, this is true. I'm certain that the name had some disadvantages and of course, the same thing held true for animal husbandry. I had always held the philosophy that what the department did or what constituted a course was much more important than the name. But I do realize that there are certain problems in this respect. And therefore, I think perhaps, the changing of the name of the department from animal husbandry to animal science is no doubt more accurate than the old name was.

Heitman: Well, I think--I don't find any fault with it either way. But I think the names were proper in their time.

Cole: Yes. This is very true. This is very true.

Heitman: Well, let's--you've had a long career with the University. I know that you've taught some other courses too, would you like to enlarge on those?

Cole: Yes. When I gave up the teaching of Animal Husbandry 110, I developed a new course in endocrinology which was known as Animal Husbandry 130, which I taught in 1952 and 1953. Then, finally, it was decided that the chairman of the department should teach the beginning course in animal husbandry. This was not my decision but I did begin teaching what used to be called Animal Husbandry 7 in 1954. I'm not too sure of what name it has now--Animal Science 1, I guess, and Animal Science 2 both have certain elements of the old Animal Husbandry 7.

Anyhow this did prompt me to become editor of a text called Livestock Production which went through two editions and now a third edition is out under the title of Animal Agriculture in which Dr. Magnar Ronning is the co-editor.

Heitman: You might be interested to know I had a letter just last week from Dr. Lubomir Itze, one of your former employees and part-time graduate student and he has moved to the Agricultural University at Prague and

Heitman: he uses your book as a textbook. I'm not quite sure how the Czechs read this (laughter) but anyhow they're using your second edition.

Cole: That's very interesting. I've heard of several in South America that are also using the text but I presume that South Americans are more likely to know English fairly well and perhaps better than some of the people in Eastern Europe.

Heitman: I would guess so. And did I understand that what used to be called the old farm school--was that the prerunner of what we knew as the two-year program?

Cole: Yes, well, first we called it non-degree then I think later the two-year program.

Heitman: Well, I guess I use those interchangeably.

Cole: Yes.

Heitman: Did you ever teach in that program?

Cole: Oh yes, yes, I neglected to point out that I taught--we had a course in animal biology. It consisted of four parts. And I taught the physiology section of that for several years in addition to my teaching of Animal Husbandry 110.

Heitman: So I suppose we should say for the record then that probably the department's most eminent scientist taught the beginning--biology course--to students who didn't even have to have a course in biology or in chemistry or anything to be admitted to the University at that time because they were in a non-degree type program.

Cole: Well, I think you overrated my capacity (laughter) but at any rate it is true that I was brought up on a farm and I think I had a feel for what students with a background such as most with a two-year program would have. So I thoroughly enjoyed the teaching of this animal biology course. I thought it was a real challenge and I thoroughly enjoyed it. Of course, you had to telescope and condense a lot of material from the conventional types of courses that are given in physiology.

Heitman: You had to be able to explain it.

Cole: Yes. (Laughter.)

Heitman: I guess that's pretty well run the gamut of the different types of courses and of course, the one you stuck with the longest and your greatest effort was the 110 course. Now just to change things a little bit. When some of the oldtimers get together, I've noticed

Heitman: that they call you "Bucket" and it seems to me that we ought to record the background of that too.

Cole: Well, I don't know that I can give you an entirely accurate background of it. It stems from two things. In the first place, I was in the Navy during World War I and of course, pails were known as buckets in the Navy. And therefore somebody hit upon the idea since I kept using the term bucket quite frequently in my general conversation with students on the Berkeley campus where I was a graduate student. They finally hit on the name of "Bucket of Cole" and therefore, the nickname. There are some people that had felt that the term might have a deeper meaning relating to my capacity for liquor or something of this sort but of course, I didn't have such a capacity (laughter).

Heitman: Well, I didn't know whether it was for carrying a bucket of beer or carrying out the excreta from some of your experimental animals or whether you were a baseball player who when he swung put his foot in the bucket (Laughter). There could be all kinds of things.

Cole: Of course, it could have been very well the carrying of excreta because I did spend several summers, when I was working on bloat in cattle, in which I did follow cows around in the field collecting all the feces and the urine to determine the amount of feed that was consumed by the cow and of course, I also had to determine how much water was consumed.

Heitman: And no one, even a livestock man who has not followed one around, really realizes how much there is, I don't think (laughter).

Cole: That's right.

CONTINUING HISTORY OF THE DEPARTMENT OF ANIMAL SCIENCE

Heitman: Well, maybe we'd better go on with some of the things that we need to cover in this interview, Harold, and one of the things that you were going to discuss was the philosophy of the department and especially the changing philosophy with different chairmen and so forth through the years. So why don't you start in and go where you will.

Cole: All right Hubert, prior to the arrival of Dr. Hart in 1926, the department was mainly concerned with feeding trials, studies of wool characteristics, digestion trials, and, of course, the inbreeding project in dairy cattle under the direction of Professor Regan, and the inbreeding project with Berkshire swine under the direction of Professor Hughes. Judging livestock shows and extension-like activities absorbed a considerable amount of time of the professors. Dr. Hart was one of the first chairmen of animal husbandry in the United States to recognize the importance of the basic biological sciences both as regards teaching and research in animal husbandry. Some departments recognized the importance of one field of science, for example, the basal metabolic studies at Penn State under Professor Armsby. Most departments took the position, however, with which I was familiar at the University of Wisconsin where animal husbandry professors had a broad training in animal husbandry but no training in depth in any discipline. To do basic studies required collaboration with members of the Departments of Biochemistry and Genetics. Only within the past fifteen years has the picture changed at Wisconsin and many other eastern institutions. The main emphasis, prior to Dr. Hart's arrival, had been on teaching and extension. More than 50 percent of the students in animal husbandry were in non-degree, for whom entrance requirements were waived. The only courses offered were so-called production courses which required a minimum of science background. I was told by a former student that one professor spent his lecture period reading to the class from the text for the course.

Judging livestock shows played an important part in the lives of the professors in this early period. For a time the department

Cole: showed animals at the state fair and up until about 1929 we showed fat beef cattle and fat lambs at the International Livestock Exposition in Chicago. I'm not too sure about that year, but I think it's approximately correct--about 1929. Do you happen to know?

Heitman: I don't know exactly but it was certainly about that time.

Cole: It is my understanding that we withdrew from competing at the state fair because some exhibitors felt that this was unfair competition. We could afford to castrate the very best lambs for competing rather than raising them as potential herd sires. The Animal Husbandry faculty members actually carried on a large part of the agricultural extension activities. There was one livestock extension specialist, Mr. Louie Rochford and one dairy production extension specialist, Eddie Gordon, both of whom were located on the Berkeley campus. There was a minimum of contact with the specialists by Animal Husbandry faculty members and thus at times the extension specialists and the Animal Husbandry faculty members worked at cross purposes, merely because of the lack of contact. This was more true of the livestock extension specialist because the dairy specialist was largely involved in the operation of the DHIA (Dairy Herd Improvement program) and thus there was no conflict of interest with members of our department.

At that time, and until sometime in the late 1950's the animal husbandry department was in charge of the Advanced Registry testing, a more sophisticated and more stringent program than the Dairy Herd Improvement Association program involving only registered purebred dairy cattle. This assignment fell to Arthur Folger. Now advanced registry and DHIA programs are integrated in large measure.

There was a continual debate as to whether the livestock and dairy specialists should be housed at Berkeley so as to be in close contact with the agricultural extension headquarters or whether they should be with the production department with which their work was most closely related. The late director of extension, Mr. B. H. Crocheron, favored the former arrangement as it allowed him to maintain tight control on the extension program.

Now I would like to say something about the philosophy of George H. Hart as it concerns both teaching and research in animal husbandry. Professor True, who preceded Dr. Hart, as chairman of animal husbandry represented the old school which adhered to the concept that courses in animal husbandry should be concerned in the management, feeding, and breeding of livestock with a minimum of concern about the application of the sciences of genetics, nutrition, biochemistry and physiology to the solving of management and breeding problems. This is entirely understandable because at that time there was a minimum amount of information on the physiology, nutrition

Cole: and genetics in domestic animals. And thus, there was very little that they could do about it at that time. But it did not occur to these early workers that they might have been building up some information on the physiology, genetics and nutrition of domestic animals.

No doubt Professor Regan and Professor Hughes, who were concerned with the impact of inbreeding in dairy cattle and swine were fairly well informed with the little information available in genetics at the time that they started their investigation. But unfortunately they did not keep up with the advances in knowledge in genetics and therefore Professor Gregory and his graduate student Wade Rollins actually did most of the interpretation of the results of the inbreeding project in dairy cattle. It was made possible for an outsider to come in and interpret the data because of the superb record-keeping of S. W. Mead and of course, because Tom Mead and Bill Regan were here to help in interpreting the data. These studies uncovered about a dozen inherited defects dependent upon a single autosomal recessive gene. At one time, Professors Regan, Mead, and Gregory held the view that they had developed a pure strain for high milk production in Jerseys but this view was soon exploded. They did have one bull which very consistently had daughters with higher milk production than those of the dams. But the sons of this bull did not have daughters which performed as well. To my knowledge, the inbreeding project in Berkshire swine was never fully recorded but, Hubert, perhaps you have more information on that than I do.

Heitman: Well, I think the difficulty was that there was no one to keep the records for the swine that there was in the case of the dairy cattle and as a result I think that the net result was essentially not much.

Cole: That is my impression. I know that Professor Hughes had a large number of other responsibilities and he himself could not keep in the close contact that was necessary and he did not have anyone comparable to Professor Mead to keep the records. Also of course, about this time Professor Hughes became very interested in swine nutrition and made some very outstanding contributions on the vitamin requirements--vitamin B requirements of swine.

Well, I've strayed from my discussion of the philosophy of Dr. Hart a little bit. He believed that not only should animal husbandry faculty be concerned with the application of knowledge derived by researchers in other biological departments but that further they should themselves develop new knowledge. Furthermore, he believed that animal husbandry faculty should be concerned in giving not only production courses but also courses in basic biological sciences of nutrition, genetics, and physiology. Had there been departments representing these disciplines, this might not have been possible. Now there are departments for each of these disciplines on the Davis

Cole: campus and the basic introductory courses for each of these disciplines are given in the basic biological sciences department. Therefore, animal husbandry faculty trained in these disciplines give advanced courses in these disciplines rather than the basic introductory course.

Next I would like to take up the question of graduate students. When Dr. Hart arrived the only graduate students that animal husbandry faculty could supervise were those working toward the master's degree in animal husbandry. The University, probably Dean Lipman of the graduate division, had ruled that animal husbandry was not a discipline but rather it leaned on several disciplines and therefore a Ph.D. could not be granted in animal husbandry. It is rather interesting, however, that a Ph.D. is still given in the Department of Zoology at the University of California even though the areas which they cover are as diverse or perhaps even more so than those in animal husbandry. Also, I should mention that Ph.D. degrees have been granted in animal husbandry for a great many years at most of the mid-western and some of the eastern departments of animal husbandry. I believe the California philosophy is preferable. To make it possible for animal husbandry faculty to guide students for the Ph.D. degree, Dr. Hart conceived the notion of graduate groups in specific disciplines involving faculty at Berkeley, at Davis, and in some instances at San Francisco. For many years animal husbandry Ph.D. students at Davis had to spend one or two years taking required courses at Berkeley.

Well, I should go on to say that he was responsible for setting up graduate groups in physiology, in genetics and in nutrition, with, as I mentioned, faculty from Berkeley and Davis and sometimes from San Francisco. Dr. Hart believed that so-called production faculty in animal husbandry should represent the department before livestock groups in the field. My first field trip was in 1942, fourteen years after my arrival on campus inspite of the fact, because of my former experience in farming that I had a strong interest in all phases of livestock production. With Dr. Hart I was invited by Reuben Albaugh in 1942 to give a talk on early breeding in rats and particularly as it related to early breeding in cattle. Reuben has told me since that Dr. Hart thought I should not be involved but, upon Reuben's insistence, Dr. Hart finally relented and let me attend this meeting near Salinas.

Now I would like to say a word about Dr. Hart's view about production and science faculty. He held strongly to the view that the department should be composed of two types of faculty: 1) so-called production faculty members and; 2) so-called scientists. Because of the fact that Goss, Kleiber, Gregory and myself were concerned with teaching basic science courses, it was natural that potential graduate students would consider that they should work

Cole: with us rather than with production faculty. This created a serious problem, and Dr. Hart felt that all faculty members should be involved in the training of graduate students. Therefore, he called Drs. Goss, Kleiber, Gregory and myself to his home one night, in the early 1940s, and told us from this time on each graduate student would have dual guidance by a member of the so-called science group and by a production faculty member. Strangely enough, he considered himself a member of the production faculty even though he was a member of two or three graduate groups. One graduate student in physiology was guided by Professor Regan and myself; however, that relationship was short-lived because the student decided to go into human medicine instead partly, I'm sure because he did not like this guidance procedure.

Another student, Bob Casady, was guided by Dr. Hart and myself. Bob was a real diplomat and got along fine with this arrangement although he did have to change his thesis project because neither Dr. Hart nor myself were enough of a chemist to make certain that Casady's negative results were correct. A biochemist could have helped in proving the effectiveness of his results which indicated that no pregnanediol appeared in the urine of the cow as a metabolite of progesterone. Well, anyhow, this guidance approach was short-lived because it wasn't sound. The people guiding the students should be based on the nature of the problem tackled and of course upon the student because this is a highly personal relationship and a student does not want to have the person guiding him shoved down his throat.

Fortunately, after Dr. Hart left in 1948, the division of faculty into production and scientific groups was abandoned under the policy adopted by Elmer Hughes and continued during my chairmanship. Now every faculty member is trained in a discipline and is capable of guiding graduate students.

Next, I would like to say something about the relationship of Dr. Hart and other faculty members. Dr. Hart's relationship with other faculty members in the early years of his tenure as chairman were in the main very good. He had a strong feeling which he frequently expressed to me, that Dr. Elmer Hughes was out to undercut him and take over the chairmanship. This viewpoint developed I'm certain solely because Elmer had built up a close relationship with the livestock industry. I've never known of a single incident in which Elmer tried to undercut George, but Elmer was a very gregarious individual and just loved to entertain livestock producers in his home.

Now a word about Dr. Hart's relationship with myself. From the time I became a graduate student at Berkeley and Davis until 1936, my relationship with Dr. George Hart was an extremely close one--almost a father and son relationship as I've stated. My wife, Cole: Jessie, and Mrs. Eva Hart were also very close and their relationship remained extremely close up to the time of Mrs. Hart's death. My close relationship with George came to an abrupt halt in 1936 upon my return from a six month sabbatical leave. Before leaving, Dr. Hart invited Jess and I to have breakfast with Eve and him on the first morning of our return. Well, not only was there no invitation to breakfast upon our arrival home but when I went to the office, I immediately asked the secretary if I could see Dr. Hart. I was informed that he would be too busy to see me at anytime during the day. This was my first awareness that our close relationship had changed. To this day, I don't know with certainty what brought about this change. I had a graduate student at that time, Francis Saunders, who was working on the number of gonadotropins present in the pituitary gland. And because of the fact that this was a very highly competitive field at this time and because the differences in viewpoints of scientists were very great, I had kept in close contact with Saunders during sabbatical leave. I presume that George felt that I should have turned over the supervision of Francis Saunders to him. However, in the meantime, Dr. Hart had taken on a number of other interests--he was very interested, for example, in the vitamin A requirement of domestic animals. He had become interested in brush burning and of course, the nutritive value of forage on the range.

Well, to this day I'm not certain exactly what the misunderstanding between us was. But I felt certain knowing George's characteristics, that if he wanted to tell me, he would have told me without my asking him and therefore, I never did ask him about this problem. I'm confident that he felt hurt that I did not ask him to take over Saunders direction in my absence but I can't be certain if this is correct. There may have been something entirely unknown to me.

There's one point I'd like to make that perhaps would clear up some misconception. I pointed out my relationship with George and our problems in the last few years. However, I want to make it very clear that I always have had and still have the greatest admiration for George H. Hart. I recognized his great talents and I recognized that he also had faults as all of us do, of course.

Heitman: I don't know whether you knew that George Hart, compared to faculty members in his time, was fairly well to do. And do you remember when--we don't do it quite as much anymore but how we used to share automobiles--university automobiles--to go to western section meetings and when I first came back as an assistant professor, George Hart liked to go to the western section meeting. And very often he was in that car and I don't know whether you ever knew but when people like Glen Lofgreen and I would go with him, on the way to those meetings he would pay our motel bill and our food and he would say I can afford this and you young people can't but when you get to the

Heitman: meetings you're on your own. And so that's the kind of a person he was. Also, as you know I was president of Davis Little League for some years and very active in it. George Hart was one of the very regular attendants in going out there and watching the kids as they played Little League baseball.

Cole: I didn't know that.

Heitman: So he was an unusual man and times have changed. When George Hart was chairman of the department and you went in to see him, you knew you were talking to the chairman.

Cole: Yes.

Heitman: This isn't quite the same anymore.

Cole: (Laughter.)

Heitman: Times have changed.

Cole: I'd like to say a word now about my relationship with other members of the department. I collaborated with every one of the professors who were here on the faculty in 1928 and before 1935 with the exception of Jim Wilson and Paul Gregory. And the fact, that I did not collaborate with them was purely fortuitous. In other words, we were good friends although my friendship with Paul Gregory dimmed somewhat in the later years of his tenure on the faculty. I collaborated most extensively with George Hart and Harold Goss but I also collaborated with Max Kleiber, Harold Guilbert, Bill Regan, Tom Mead, Elmer Hughes, Carroll Howell and Bobby Miller.

Now I would like to say a word about Elmer Hughes as chairman in 1948 to 1951. This was a very happy period for me. There was never a disagreement between Elmer and myself. Apparently, Max Kleiber did have some misunderstanding but I never completely understood it and never asked what the reason was. Elmer Hughes, unfortunately, dropped dead in his office while having an interview with Dr. Hart in the early spring of 1951. Following his death, Harold Guilbert was made acting chairman but his tenure was only for a few weeks because he became ill and I took over as acting chairman then and in the following year was made chairman up until 1960.

Heitman: Well, Professor Cole, before we go on and discuss some of the things that occurred while you were chairman of the department, you spent some time mentioning some of the early members of the faculty of the department. The department has grown considerably in size, just like the campus has to a certain extent, and I'm sure some of the increase occurred during the time we have now discussed. Would you like to add anything to this about some of these newer faculty members?

Cole: Yes, well before Dr. Hart left the department, he felt that there were a number of faculty members who were reaching--coming close to the time for retirement. And Dr. Hart felt it was very important that a replacement be found for these people so that they would in a sense have a chance to be indoctrinated with the philosophy of the department and also to make contact with the livestock industry. During this period, Dr. Hubert Heitman was brought in in 1946 as an eventual replacement for Professor Elmer Hughes. P. T. Cupps was brought in in 1947 as a replacement for Carroll Howell. W. C. Weir was brought in in 1948 as a replacement for Robert F. Miller. Glen Lofgreen and Jim Meyer were brought in in '48 and '51, respectively, but I'm not exactly certain as to who they were to replace. Perhaps they were to replace Dr. Hart. Dr. Carroll was brought in not as a replacement--well, yes, as a replacement for Harold Guilbert, and Wade Rollins joined the department in about 1948 not as a replacement for anyone. I think as an additional staff member, as I recall. Dr. Nate "Pat" Ralston was hired to replace Bill Regan.

Heitman: I think so. Glen Lofgreen likes to say that he's the person who replaced George Hart (laughter) but I think that's just his FTE substitution on a piece of paper.

Cole: Yes, I think this is correct.

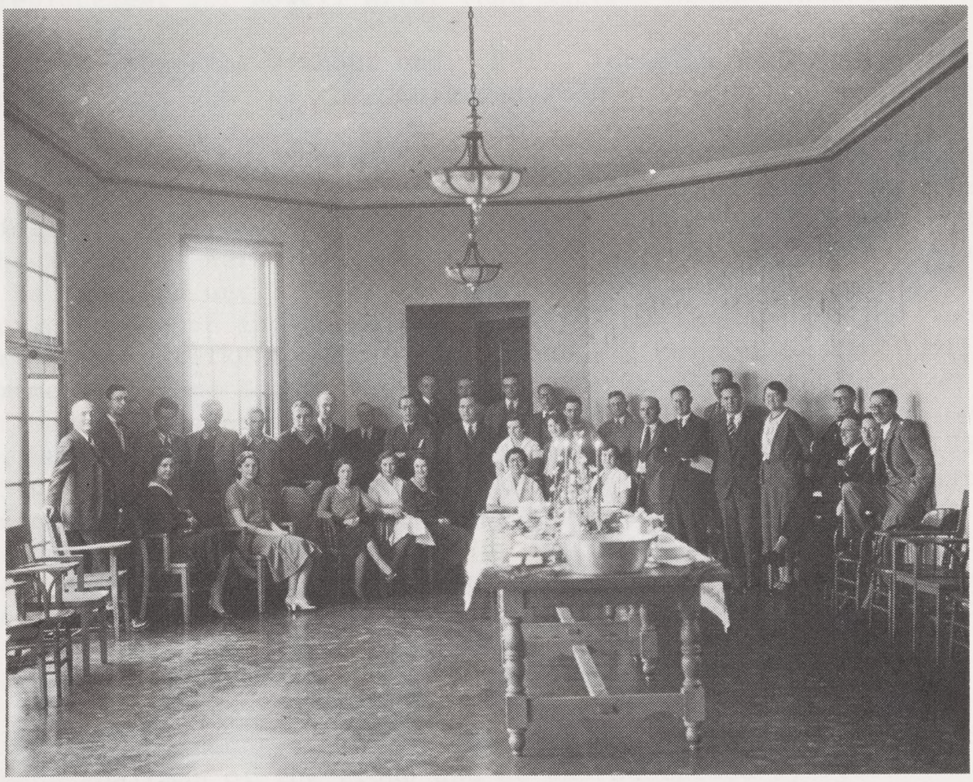
Heitman: It would be pretty hard to replace George Hart.

Cole: Yes (Laughter).

Heitman: Okay, now do you want to go on and--

Cole: I would like to say a word about these people. All of these people were trained in a specific discipline with the possible exception of Pat Ralston. Pat received his Ph.D. in Animal Husbandry at Cornell and lacked the depth of training in a given discipline. He left within a few years probably because there were problems in obtaining promotion. He was succeeded in 1950 by Dr. Robert C. Laben who is a well-trained geneticist. The solid training of these new men helped a great deal in changing the philosophy of the department because no longer did we have this problem of having persons on the faculty who were not allowed to take on the guidance of graduate students. They also, of course, greatly bolstered the department in some fields of science which had not been well represented prior to this time.

Heitman: I think maybe too when you were reading the names or mentioning them that Professor Cupps, even though he originally came in to work in the horse area--and did for a year or two--eventually moved on into the dairy cattle research area where, of course, he's become very well known.



Taken in either December 1931 or 1932 in 301 Animal Science. Tracy Storer was the photographer. Standing (men) left to right: (1) George H. Hart (2) Dougherty—Poultry (3) ? (4) Max Kleiber (5) ? (6) Johnson, H. Goss' technician (7) Lefty Caldwell, H. Goss' technician (8) Wes Campbell, H. Goss' technician (9) ? (10) J. F. Wilson (11) W. M. Regan (12) ? (13) C. E. Howell (front) (14) H. R. Guilbert (15) Stanley Freeborn (16) ? (17) Wylie Lloyd—Poultry (18) E. H. Hughes (19) P. W. Gregory (20) Arthur Folger (21) J. E. Eckert (22) Miss Ulven (23) R. F. Miller (24) H. Goss (25) F. J. Saunders (grad student) (26) H. H. Cole. Sitting left to right: (27) Mrs. Dorothy Thomas, Department Secretary (28) Miss Fern Elliot, Zoology Secretary (29) ? (30) ? (31) ? (32) Beryl Marshall Wilson (33) ? (34) the present Mrs. Otto Ruban (35) Mrs. Frank Campbell.

Cole: This is true, yes. He did shift over. His shift, no doubt, was related to Pat Ralston's leaving. I don't think they provided for any replacement for Howell after than until I became chairman.

Heitman: That was a long time without one. Well, let's see why don't we go on now and discuss some of the "Cole years?"

Cole: Before discussing the "Cole years" as chairman, I would like to go back to say a little more about the Graduate Group in Comparative Physiology. Professor J. M. D. Olmsted, chairman of the Department of Physiology at Berkeley, was the first chairman of the group--a post he retained until his retirement in 1953. The membership of the original group included members of the physiology and biochemistry departments at Berkeley, Professor Agnes Fay Morgan, Chairman of the Department of Home Economics, Professor K. F. Meyer, Chairman of the Department of Bacteriology, Professor Alva Davis in Botany and Cole, Goss, Hart and Kleiber from Davis. I served as chairman from 1953 to 1966. In 1959, I wrote to Dean Maurice Stewart of the Graduate Division requesting that the name be changed to Animal Physiology and the request was granted. On May 20, 1966, Dr. James Boda moved that the three-campus group be disbanded and that a Graduate Group in Physiology at Davis be established. In a meeting on June 6, 1966, I announced that our petition to establish a Graduate Group in Physiology at Davis had been approved. This change from Animal Physiology to Physiology was made in order to accomodate members of the Human Physiology Department at Davis. On August 5, 1966, Dr. James Boda was made chairman of the group and he was succeeded by Professor Loren Carlson, Chairman of Human Physiology on June 9, 1970.

EVENTS AND PROBLEMS OCCURRING DURING COLE'S TERM AS CHAIRMAN OF THE DEPARTMENT

Cole: For the most part I thoroughly enjoyed my years of chairman from 1951 to 1960. And I brought in the following people: J. M. Boda, who had been one of my graduate students, to teach the course in physiology; M. T. Clegg, was brought in a little bit later in '52 or '53 to teach the course in endocrinology; Dr. Magnar Ronning was hired as a dairy nutritionist because we really had no one who had a sound training in this area. It is true that S. W. Mead--Professor S. W. Mead--had made some very significant contributions in nutrition but really this was not his basic discipline. Dr. Eric Bradford, a geneticist was brought in to work on genetics of sheep and Dr. Irving Geschwind, as a replacement I presume for Dr. Goss. Dr. Robert Loy somewhere in the mid-fifties was hired to replace Professor Carroll Howell. All of these people distinguished themselves with the exception of Dr. Loy who did not receive tenure. I'm not too certain as to whether this--his dismissal was a mistake or not. He came to us from the University of Wisconsin. He was a student of Dr. Casida there and he had already published four articles in the Journal of Biological Chemistry before he arrived, and I felt that he had a very bright future. But when Bob got here he, of course, was in charge of the horse barn and also involved in not only one research project but in a series of projects which meant that he did not--was not able to complete any of them. Therefore, most of the work that he did while he was at the University of California for about seven years was not published until after he left here. But at any rate it was the fact that he had not published which resulted in his final dismissal.

Now I'd like to say a word about our relationship with the livestock industry. Because of the fact that I'd been brought up on a farm and had actually farmed for several years, I thoroughly enjoyed my contact with the livestock industry and spent a good deal of time in the field. Also I felt, contrary to the view which Dr. Hart had previously held, that every member of the department should become acquainted to some extent with the livestock industry. That was not to say that some people should not be more concerned

Cole: with contact of the livestock industry than others but I believe it is extremely important that they do rub shoulder to shoulder to speak with members of the livestock industry. I believe this gives them a better understanding. They get to know the problems of the livestock industry and, I believe, it is to the benefit of both the department and of the industry itself.

One of the most interesting times that I had was at a meeting with the California Cattle Feeders in which they brought up the question as to whether the University was ever going to build a feed mill and a feedlot. Well, this was in the mid-fifties and the University had told me in no uncertain terms that they were not going to be able to supply the money because the University was heavily involved in establishing several new campuses. I repeated this to them at this meeting and said that I thought there was no chance unless there were outside funds that we'd ever get the money for this activity. Well, a man whose name escapes me got up and said, "Well, I will give $5,000 to start this off if the rest of you will come along with me." In the matter of about a half hour, we'd raised $75,000 for a new feed mill and this was eventually raised up to about $165,000. It's true that some other organizations also participated in this fund raising including the California Hay, Grain and Feed Dealers and I think some of the meat packers also donated funds. But the California Cattle Feeders provided the leadership and donated the major amount of money which resulted in the erection of a feed mill and then the University of California did supply the money to build a feedlot. This has been of great benefit to us in conducting applied research in nutrition, physiology and genetics.

Another activity with which I was involved was, of course, in establishing the feeding trial budget. We'd always been in hard straights to carry on practical, applied feeding trials in any class of livestock because of the tremendous amount of money that was involved--first, in purchasing the animals and secondly in buying the feed to bring them up to the point where they'd be marketable. Therefore, I approached Vice President Wellman on this point. At first he was adamant against setting up a feeding trial budget because he said that this would immediately result in second class research, simply because you would be thinking more about the money you were going to make than you would be about the results of the research. However, it hasn't worked out this way. After some time, I was able to convince him of the correctness of this and, if it were not for the feeding trial budget, I'm sure our activities would be very different. I'm not sure but perhaps even your activities have been somewhat involved in this respect.

Heitman: Swine research is just about all supported that way now. In fact, I would guess--I would say that Vice President Wellman's fears were well-founded but they certainly have not been realized at all and the

Heitman: livestock feeding trial is supporting all kinds of research and it certainly has not been abused as far as I'm aware.

Cole: No, it has meant, of course, that other parts of the budget, that is, the regular animal husbandry budget has had to be used to take up the slack. In other words, for example, if you're going to feed out a group of steers and it's necessary to take certain samples of the carcasses of these animals after slaughtering, somebody's going to have to pay for that loss. And the regular animal husbandry budget does this. But we can spend--we have used up to around two hundred thousand dollars a year, I think.

Heitman: I think it's more than that now.

Cole: It has made it possible for us to carry on activities that we'd never been able to do. I remember when with Dr. Hart and Professor Guilbert, we carried on a study on the effect of pregnancy and of spaying on the rate of gain of beef heifers. We were able to have, I think, about five animals for each group, whereas today we could have twenty or thirty animals to carry on this in a much more scientific manner than we were able to do at that time. But we just did not have money to buy and feed animals for this type of activity.

Heitman: If the budgeted money the department was receiving in regular funds had remained the same without the livestock feeding trial, the department would not exist as we know it today.

Cole: Yes, this is very true. Dr. Wellman originally suggested to me that, "Well, if you need this extra money for these activities, just put it into your budget." But this would have meant that it would have more than tripled the size of our budget which you couldn't possibly expect to get.

Heitman: Well, we kept asking for things in the budget and they were always cut out.

Cole: Deleted. Of course, yes. Well, because of my interest in the livestock industry, I was given the Golden Fleece, I don't remember the exact year--somewhere in the 1950s--by the California Wool Growers.

Another activity with which I was involved was the A. H. Memorial Fund which was a fund set up to memorialize prominent people in the livestock industry and prominent people in the Department of Animal Science, who had contributed significantly to livestock production activities. I had hoped that this would serve as a cementing influence between the department and the livestock industry but further that it would result in the raising of a sufficient amount of money to carry on a lot of activities that otherwise could not be supported. I must confess that this has not been achieved. We do--

Cole: we are able to do a certain number of things with the interest on the money that has developed--somewhere between ten and fifteen thousand dollars, I think at the present time--but I was hoping that we might have a fund eventually in the order of a million dollars. I think, perhaps, what is needed to get this off of the ground is to set up a committee of producers in which they could stimulate interest in the development of this fund. Another reason, I think, it hasn't gone over large is the fact, that even though we might have a little write-up in a special memorial book about each member who has been memorialized by funds given for this person there is no really objective way of perpetuating the name of the person memorialized excepting through this memorial book. And this is the limitation. Because of this there have been attempts when some faculty have died to set up a special memorial fund for this individual. For example, I know a very big drive was made for Elmer Hughes, and I don't know how much money was raised--I'm not sure just what that money is used for now. You probably know.

Heitman: There was over ten thousand dollars raised and this was used to fund scholarships for undergraduates--entering freshmen. But this antedated the memorial fund. I expect it's one of the things that led you to make your decision.

Cole: Yes, I knew what a great effort had gone into raising this money and I knew drives of this nature could not be conducted whenever a faculty member died. And, of course, one of the things I had to promise when I set up this fund was that we would not make a great effort to raise money. But I think we could go as far as having a producers committee to more or less stimulate activity--let the department know when someone is deceased and they might actually become involved in seeing that this person is memorialized and recognized in a proper manner.

Heitman: Harold, thinking about the things you have caused to happen in the department, another one, I think, that should be mentioned is the formation and functioning of the livestock advisory committee which certainly has played a part in our departmental program now for some years.

Cole: Yes. I think that this has been an important development and has tended to keep us in contact with the livestock industry. One of the problems we had when we initiated this livestock advisory committee was whether or not the department of veterinary medicine and more recently--well actually the School of Veterinary Medicine--should be involved. But since diseases are such an important phase of management of livestock, we felt that they should be involved. This has been somewhat of a problem in some respects but by and large I think it has been successful and I think the setting up of these committees in allowing the livestock producers to give us their ideas

Cole: as to what we should be doing in the way of applied research, has a very beneficial effect.

Heitman: I think the veterinary school's probably played a little less of a role in it the last several years than we liked--in previous years too--because afterall we do all represent the livestock industry and the livestock people we serve are essentially the same ones that they do.

Cole: Yes that's correct. There's--it would be very unwise to have separate committees, I'm sure.

Another item that I would like to take up is this question of judging team contests. For many years, before I arrived in Davis, the Department of Animal Husbandry trained a judging team which competed at the Portland Livestock Exposition in Oregon. However, at the time that Dr. Hart arrived, he felt, I think, that this was an unnecessary activity and the judging was soon discontinued. I can't remember the exact year.

Heitman: I guess it probably was about in 1931 or '32 something like that.

Cole: I would think that that's right. I remember that Ken Wagnon was a member of the team but I don't remember when he graduated.

Heitman: Dan Danielson was on one of the last ones.

Cole: Was he?

Heitman: Yes.

Cole: Well, I felt that this was a very important function. Students received a big lift out of an activity such as this. I knew this from the fact that I'd been on both the livestock judging team and the dairy judging team at the University of Wisconsin. Consequently, I thought it should be revived but I felt at the same time that we had now progressed in our knowledge of selection of livestock to the point where we should not rely solely upon conformation in determining the value of a sire, for example. Therefore, I contacted the people in the western states and they were agreeable to such a contest and it was held at Davis for the first year. In order to get this thing going properly, we invited Dr. H. H. Stonaker, a geneticist, from Colorado State College (now the Colorado State University) and asked him to more or less oversee both drawing up the inheritance portion of the contest as well as the conformation type of the contest. I thought it was very successful. Our teams won (laughter) for several years in a row and I presume that this had some influence on the lukewarm attitude of some of the other western institutions. Also, apparently one year we had someone in charge of this event who was

Cole: somewhat less enthusiastic than I had been and it just gradually died out. But I still think it is a very important function and the students would get a big kick out of it just as they do out of Little "I".

Heitman: I think one of the problems, Harold, was that the discussions on the selection contest, as we called it, were held at the western section meetings of the Society of Animal Science and the decisions that were made there were made by a broad-based group of people which included the department chairmen as well as the faculty across the ranks--professors, associate professors, and assistant professors. The contest, of course, it was hoped would be adopted by the Cow Palace. The Cow Palace every year has a meeting of the coaches of their judging teams and except for Davis and one or two others, the coaches are always junior members of the faculty and in almost every case, they were not trained in the science of genetics and you can guess what their recommendations were to the Cow Palace and let's face it, they aren't about to do themselves out of a job.

Cole: I think you hit the nail on the head, Hubert. I think that this is exactly the problem we were up against. However, I feel very strongly that this is an activity which we should sponsor and I have felt for a long time that livestock expositions should be organized in a manner which would recognize the importance of inheritance in judging livestock. For example, we know that no one goes out to pick a herd sire, a dairy bull, for example, solely on the basis of conformation alone. And conformation is going to tell you very little about the prepotency of that bull as far as milk production of the daughters is concerned. Consequently, Reuben Albaugh and I, when the state fair was about to be reorganized and moved out to the new site on Arden Way, made a valiant attempt to have a revised system of judging at the fair. However, at about the same time the governor of the state--I guess it was Governor Reagan--appointed a committee in charge of the livestock show comprised entirely of people who had been showing cattle and other livestock for many years. Naturally, they didn't want to make changes and get into this new area. Consequently the idea died. I still think it has merit and should be pursued further.

To pursue this point further, I would like to see beef bulls judged on the basis of rate of growth, the conformation, and, finally, the quality of the carcasses of an unselected group of progeny. These progeny could be exhibited at the fair along with the sire and then judged and slaughtered to obtain information on the quality of the carcasses. This would involve a revolution of livestock judging as we know it today but the current system is antiquated, out of touch with the science of genetics and, for the most part, without educational merit. Much the same system could be used for other meat producing animals. In dairy cattle, again, I would recommend that sires be judged on the basis of conformation and upon the milk production

Cole: of an unselected group of daughters. Conformation judging should be limited to those traits which have economic merits such as soundness of legs and feet, body size, size and shape of udder and teats. Pendulous udders, for example, are subject to injury, small teats present problems in milking as do unbalanced udders.

These systems will only work if animals are supervised before reaching the fair but I can see no other way of making the livestock expositions truly educational.

Now, perhaps I should say a word about some special problems with people in the department. As I mentioned above, my contacts with the people in the department when I was chairman were I felt excellent. I got along fine with everybody. There were one or two exceptions. And one of the exceptions was Max Kleiber.

Even though he and I had collaborated on a number of projects and put out six or eight papers in which we were most amiable, I found that it was a little bit different when I got into an employer relationship with Max. I was about to try to find somebody to replace Max when he retired. And after discussing this with some of the nutritionists and the biochemists, I had decided on a Dr. Schambye from Copenhagen, Denmark to replace Max. Well, Max heard of this and he was very interested in having Jack Luick, his graduate student, appointed to this position. I had great respect for Jack Luick but I did not think he in any way could replace Max Kleiber and therefore I rejected the idea. Well, as a result of this Max wrote to Dr. Schambye and told him that he should not come to Davis because he would not have academic freedom here. This, of course, killed this appointment. However, we were very fortunate later, after I had retired as chairman, to get Dr. Lee Baldwin, who is, I feel, carrying on in the tradition of Max Kleiber. Dr. Kleiber had the very laudable trait of supporting his colleagues but, in my view, in this instance he went beyond reasonable limits.

Another problem I had was with Dr. Paul Gregory. Gregory developed what I considered to be a very difficult personality in his later years. He had a very different viewpoint from that of Lush and Stonaker and other geneticists regarding the genes involved in dwarfism in beef cattle. This had become a very big problem in the beef cattle industry and Dr. Gregory, with my help, set up a herd of dwarf carrier cows--that is cows which had produced dwarfs--to study this project. Anyhow, during the course of the studies, he developed a viewpoint which was contrary to that of Dr. Stonaker and Dr. J. Lush. And as a result--because of his--because several of his papers were rejected or because the referee recommended major revisions of his manuscript, he thought Lush and Stonaker were out to get him. And in the meantime, he developed evidence which he interpreted as indicating that our UCD line of Herefords--the so-called Rover line--

Cole: carried the dwarf gene. A large number of cattlemen who had purchased our bulls and advertised them to be dwarf-free were miffed when Gregory stated publicly that without equivocation the Rover line carried the dwarf gene. They asked me to do something about it. But Paul was adamant. In my view he never did obtain evidence that the Rover line carried the gene but he did do irreparable damage to the department's relationship with the purebred beef industry. His collaboration with Wade Rollins was terminated because of a difference in views regarding the efficiency of Paul's profilometer to recognize heterozygous cows. The profilometer was a device which recorded the shape of the cow's head and presumably if there was a certain raised area on the head this was supposed to indicate that the cow carried the dwarf gene. He developed this profilometer which determined the extent of this raised area.

Well, Wade, in essence, did not agree that Paul had sufficient evidence to prove the accuracy of the profilometer. Paul became very irritated as a result and his relationship with Wade terminated. He also dismissed at the same time Mrs. Mel Green because he thought she was doctoring his data. Well, Gregory with Dr. Clark of the U.S. Department of Agriculture, put out a bulletin on the profilometer and unfortunately they did not give any raw data in this bulletin. It is rather suprising that Dr. Dempster, who was then in the Genetics Department at Berkeley, and who reviewed it, did not recognize this defect because the lack of raw data made it impossible to decide upon the effectiveness of the apparatus. It turned out that the width of the hair could throw an animal into the dwarf carrier gene group. The instrument is no longer used by anyone to my knowledge.

CAMPUS ACTIVITIES

Cole: Next I would like to say a word about my campus activities. During my tenure at Davis I held membership on every major campus committee and for two years was chairman of the Davis Division of the Academic Senate. This made me, of course, a member of the Academic Council while Dr. Kerr was president of the University.

For about ten years I was chairman of the three-campus physiology group consisting of members from Berkeley, Davis and San Francisco. The only previous chairman had been Dr. J. M. D. Olmsted of the Physiology Department at Berkeley. At about the same time that I relinquished the position, it was decided that the three-campus organization was no longer necessary. Currently, that is 1976, there are about sixty faculty on the Davis campus who belong to the physiology group and in the beginning of 1933 when the group was first originated there were only four--Dr. Hart, Dr. Kleiber, Dr. Goss and myself.

At about the time that I reached emeritus status, I was asked to be chairman of the Department of Animal Physiology. At first I vigorously resisted the formation of this department but eventually I saw it was inevitable. My opposition to an independent physiology department was based on the assumption that this department would take over the basic science course in mammalian systemic physiology and eventually it would drain the production departments of physiologists. Now the latter has not happened because of the expanding student body; however, had the number of students gone down, I'm sure this would have had an adverse effect on us.

I declined the invitation to become chairman of animal physiology because I had had close ties with the Department of Animal Husbandry for so many years that I preferred to stay there. Also, I had an active research program in progress which I didn't wish to abandon to take on another administrative position.

EDITING RESPONSIBILITIES

Cole: One other activity I would like to mention is editing. As I have mentioned above, I edited two editions of the book Livestock Production published by W. H. Freeman and Company. With Dr. Magnar Rönning, I edited Animal Agriculture also published by W. H. Freeman in 1974. Dr. Perry T. Cupps and I have just completed the editing of the third edition of Reproduction of Domestic Animals published by Academic Press. I also edited the book, Gonadotropins, published in 1964 by W. H. Freeman and Company. In addition, I had been associate editor of the Journal of Animal Science and in 1968 was appointed editor by the Society for the Study of Reproduction of the journal which the editorial board and myself named Biology of Reproduction. This has proved to be a very popular journal.

Heitman: I guess you were the first editor.

Cole: Yes, I was the first editor of this. I gave it up two years after taking it on, however, so that I could edit the book Animal Agriculture. I found that I couldn't do both at the same time because at the time the Society for the Study of Reproduction had limited funds and consequently I had very little assistance.

Heitman: Is the Animal Agriculture book meant to replace Livestock Production?

Cole: Yes it is. A rather interesting thing, however, is that there--that the sales are still very good of the Livestock Production text. I don't know why this is true.

Heitman: But it is not envisioned that it will ever be revised?

Cole: No, not expected to.

FAMILY TIES

Cole: Now, in closing this section I would like to say a word about my family life. I've already mentioned my mother, father, brothers and other relatives. I was married in 1929 to Jessie I. Cliff. During a stomach examination in March 1952, her heart stopped but by opening the chest and massaging the heart, the surgeon was able to restore its activity. She never came back to health, however, and two months later while still in the hospital she developed infectious hepatitis as a result of a blood transfusion and five days later, May 2, 1952, she passed away. One son of this marriage, Cliff H. Cole, is in charge of production for Red Star Yeast plants throughout the United States. Cliff was an outstanding student and leader in high school. He was also a good student at UCD and received offers of assistantships to go on with graduate work. He decided to go into industry, however, and success indicates he made a wise decision though I think he could also have been outstanding in academic work.

In 1955 I married Cynthia Clegg. Our daughter, Nancy, is now a student at American River Junior College. Nancy has a fine mind but she has never felt the urge to be an outstanding student. She has a great interest, and I think talent, in clothing design and her part-time work at Joseph Magnins is giving her a fine experience. My wives and my children have been a great joy to me and my wives especially have been most helpful and understanding throughout my career.

Heitman: Well, Harold, as a person who has lived next door to you now for my golly, something like twenty-five years?

Cole: Yes, just twenty-seven.

Heitman: I remember, of course, Jessie very well and Cliff is a fairly good-sized young man and your second family--you're a first class neighbor. You're probably a better neighbor to me than I am to you.

Cole: No, I don't believe that (laughter).

Jessie Inga Cliff Cole (1900-1952) 1935.

Harold H. Cole, 1925.

Heitman: And I will never forget the member of your family that you didn't mention. One cocker spaniel dog named Rod who was a neighborhood pet and who our family liked very much.

And now on a personal note there is one other thing that I think you should tell the people who are going to listen to this history in the future. You probably don't remember--one time you came over to my home and we were sitting on the davenport talking, I don't know about what, and my youngest son who my golly, he's twenty-seven years old now and a practicing attorney in Southern California, was just a little boy--was sitting there on the floor staring at your face. And all of a sudden you very naturally asked, "Bill, what's wrong?" And he said, "I was just trying to figure out why you have a mustache." (Laughter) And I'll never forget that because you know it left you just a little bit speechless for a minute!

Cole: (Laughter) I'm sure it did!

Heitman: Well, Professor Harold Cole has led a very distinguished life in the University. He has won many awards. He has talked about a few of them during the interview that I have had with him. I'm sure that in the next segment, you will talk again about awards. I guess at the time you retired, the University--is that when they awarded you an honorary LL.D. degree?

Cole: Yes.

Heitman: Which from the University of California I'm sure we'd all agree is a very high honor. To me it's been a privilege to talk with you because as an undergraduate student and then coming back onto the faculty, a number of the people whom you've mentioned of course, I knew and too, I think about the continuity back through the days of Harold Goss, with his associations traced back almost to the beginning and then on through people like Bobby Miller and of course, George Hart and yourself. I had all of you as teachers. I have my own opinions of all of you in their own diverse ways and how they affected my life because if it hadn't been for Bobby Miller, I never would have met my wife, for instance, because he was chairman of the Davis Joint School Board or whatever it was called in those days, and hired my wife and brought her to Davis, as a public school teacher.

So it's been a pleasure and I'm sure it's all well worthwhile.

Cole: Hubert, before closing our interview I would like to say something concerning developments in the Department of Animal Husbandry which followed my chairmanship. As concerns the department, these years in many respects became the "golden period" for both teaching and research. Professor James H. Meyer succeeded me as chairman. It was

Cole: during his chairmanship that a grant from NIH was requested for construction of the research facility. This request was granted and I was most appreciative when this facility was named the Harold H. Cole Facility in my honor in 1969. You were chairman of the committee drawing up this request. This facility included a meats laboratory which made it possible to refine many of our basic and applied studies relating to meat production. Also, it provided quarters for nutritional and physiological studies and, finally, better housing for small animals.

Professor Meyer brought in Dr. R. L. (Lee) Baldwin whose studies on mammary gland control and function have added prestige to the department. He also brought in Dr. Harold Hintz to replace Bob Loy. Harold left after a few years to return to Cornell University.

Our department has provided a reservoir for administrative talent and in 1963, Jim became Dean of the College of Agriculture and, of course, later Chancellor at UCD. Earlier, Dr. Hart left his post as chairman of the department to become the second dean of the School of Veterinary Medicine and Professor William (Bill) Weir took on the title of Dean of Students in 1957.

You, of course succeeded Jim as chairman of the department. During your tenure, the strong support of research by NIH was continued and your tight control of the budget made possible the renovation of many laboratories in the department. It was during your tenure that Tom Stowers was brought in as laboratory assistant in charge of ordering and receiving, a position made necessary, in considerable measure, by the increased research activities under research grants. Later, of course, his title has been upgraded and he in essence has charge of the budget. During your tenure, Dr. C. R. (Bob) Ashmore was brought in to complement the studies on meat research which Floyd Carroll had initiated. Floyd was one of the first to determine the influence of genetic, physiological and nutritional factors on meat quality. His studies were made in collaboration with the Departments of Food Technology and Home Economics.

During your tenure, Dr. Graham Gall was brought in as a replacement for Paul Gregory. Graham has continued his basic genetic studies and also has developed a strong program on genetic improvement of trout.

You also brought in two prominent nutritionists from Australia, James G. (Jim) Morris and David W. (Dave) Robinson. Jim's main interest has been ruminant nutrition to fill the gap left when Jim Meyer became Dean of Agriculture. Jim's studies have involved range studies at the Sierra Field Station at Marysville. Dave's

Cole: nutritional studies have centered on single-stomached animals. In addition, he served two years in the Far East with FAO and has just been appointed Associate Dean of Agriculture.

Bill Garrett was brought back to Davis from Imperial Valley to do basic nutritional studies and to concentrate on feedlot nutrition. He was replaced at the Imperial Valley Field Station by a former Davis graduate student, Verne Mendel.

Dr. Magnar (Mag) Ronning was chairman of the department from 1968-1973. At the end of this period he was appointed Associate Dean of the College of Agriculture in charge of the Animal Sciences program. Mag, while chairman, brought in Warren Evans in charge of horse production and Gary Moberg, the former as a replacement for Bob Loy and the latter as a replacement of M. T. (Tran) Clegg.

It was either during Dr. Ronning's tenure or Dr. Bradford's tenure as chairman that Dr. Boda left the department to become Chairman of Animal Physiology, Dr. Mendel left to replace Dr. Boda and Dr. Weir left to become chairman of the Department of Nutrition. On top of that, Hubert, you were appointed assistant to the Vice Chancellor of Academic Affairs. I know of no other department on campus who has lost so many faculty to administrative posts.

On top of this, Hubert, I understand that you have continued in charge of the department's graduate student program. Even during your present sabbatical year in residence I understand you are continuing with this latter function as well as serving on some administrative committees. I am amazed that you have been able to devote any time to your long-time and fruitful research program on environmental physiology.

Dr. Bradford's tenure as chairman has extended from 1973 to date (1977). Five more faculty members have been recruited: Ed Price (animal behavior), Kelvin Kuong (biomathematics), Joan Macy (microbiology), John Pollak (genetics) and Nate Smith (dairy nutrition). This represents an extension of the field of teaching and research in the Department of Animal Science but does not truly represent the total extension as a perusel of the General Catalog, Davis for 1976-77 will reveal. The original function of the Department of Animal Husbandry was to teach and do research relating to our large domestic animals. Now additional areas include management of laboratory animals, management of nonhuman primates and husbandry of semidomestic and exotic animals. Also new fields relating to domestic animal management will be introduced by new faculty such as microbiology and animal behavior. All of this represents, I think, a logical extension of departmental activities but I would hope that this extension would not dilute our interest in and devotion to the livestock industry.

INITIAL RESEARCH: ASPIRATIONS AND INTERESTS

Geschwind: I am going to discuss with Professor Cole today his scientific career. And I'll start off, Harold, by asking you just what gave you an interest in science?

Cole: Well, Irv, my interest in science was somewhat fortuitous. As I've stated earlier when I came to the University of California in 1924, my objective was to obtain a master of science degree to acquaint me with the special problems of livestock production as they relate to California conditions and then it was my intention to go back into livestock production. However, because of contact with two people I developed an interest in research. The first was Dr. George H. Hart, who at the time of my arrival was acting chairman of the Department of Veterinary Science at Berkeley. And the other was with Professor Herbert MacLean Evans who was chairman of the Department of Anatomy.

Geschwind: Can you tell me what was the attraction in coming to Berkeley in the first place? How had you heard of it?

Cole: Well, I really hadn't heard that much about it but I was interested because of an allergy problem of getting into a new environment and somehow or another I had heard of the establishment of the College of Agriculture at Davis and when I got here I found out that in order to get a master's degree that all of my course work had to be taken at Berkeley because there were no appropriate courses on the Davis campus.

Geschwind: So you did come to Davis first in 1924?

Cole: I'm not really certain whether I actually visited the Davis campus or whether I learned--I think I had learned through correspondence--that in order to take graduate work the Graduate Division was located on the Berkeley campus and therefore I went to the Berkeley campus where Professor Kennedy--of the Agronomy Department--was then Dean of the Graduate Division. And it was rather interesting that the Graduate Division was then housed in a room about the same

Cole: size that we're now in--about 14 x 14 thereabouts as compared to the present situation of the Graduate Division on both the Berkeley and Davis campuses where it becomes a very--where it occupies most I guess of a given floor (laughter).

Geschwind: Were there a great many graduate students in agricultural subjects at the time in Berkeley?

Cole: No, there were relatively few. I can't say how many were in entomology and plant pathology--those were the two big departments present at that time that housed the graduate students. I think perhaps I was the first graduate student in animal husbandry to the best of my knowledge.

Geschwind: Had you heard of any of the people in animal husbandry at Davis while you were back in Wisconsin?

Cole: No. I didn't know any of them.

Geschwind: You simply knew that there was a Department of Animal Husbandry at Davis?

Cole: Right.

Geschwind: I see. All right, now you came to Berkeley then and how did you make your acquaintance with Professor Evans? I can understand how you would have seen Professor Hart but how did you run into Professor Evans?

Cole: Well, it was Professor Regan who was my major advisor and Professor Hart who suggested since I had an interest in research in reproduction, they suggested I contact Dr. Evans. And they also suggested that I take the beginning course in microscopic anatomy which was given under Dr. Evan's supervision.

RESEARCH AT BERKELEY AND UNIVERSITY OF MINNESOTA

Geschwind: Now, when did you actually first start research?

Cole: Well, as far as my research for my master's degree I began sometime late in 1924. But as far as my research with Dr. Evan's department it began in 1925 when in conjunction with the course in microscopic anatomy it was necessary to do a research project. I presume this probably held over to the time when you may have taken microscopic anatomy.

Geschwind: Yes. I don't know whether it's still being done but it was done in my day as well. What sort of research project did you get started on with Professor Regan initially?

Cole: My research was the role of calcium in reproduction in the cow. This was suggested to me by Dr. Hart and Professor Regan. They had been greatly interested in some studies which had been made at the University of Wisconsin indicating that calcium might play a special role in reproduction. And therefore we took some cows--I put some cows on a low calcium ration and we even fed them--we gave them calcium-free water. We obtained this by obtaining the effluent from the steam plant on the Davis campus. It was the only chance of getting a major supply of calcium-free water. This research--all this research did was to show that an animal could be placed on a low calcium ration with no evident changes in the estrous cycle or other manifestations in the cow for at least a year's period. That's essentially what we found.

Geschwind: The animals you worked on were up at Davis?

Cole: Yes they were at Davis.

Geschwind: And where did you live during this period of time, in Davis?

Cole: I lived both at Davis and at Berkeley. I had to continue my course work on the Berkeley campus but I was here during the fall of 1924

Cole: to complete--to get my research under way and then in the spring of 1925 I went back to the Berkeley campus.

Geschwind: And at that time you started work with Professor Evans?

Cole: That's correct.

Geschwind: Now, he assigned you to a project?

Cole: Yes, I studied the changes taking place in the cervix of the rat during the cycle and the changes found were not very spectacular but at least my report somehow or another apparently left a favorable impression upon Dr. Evans and perhaps more particularly on Dr. Miriam Simpson, his right-hand "man", who really ran the laboratory sections. And therefore in January of 1926, Dr. Evans had received some money from the Rosenberg Foundation in San Francisco for studying the estrous cycle in the dog and he suggested that I might undertake this project.

Geschwind: Now how did you get started on this? Where did you get your dogs? Where did you find the facilities to actually run this?

Cole: Yes, there were no facilities and therefore one of the first things that we had to do was to find some place to keep the dogs. We went to see the secretary-manager of the Humane Society in Oakland and he together with Dr. Hogarty, who was the veterinarian and their scientific advisor, agreed to build these special facilities at the Humane Society quarters in East Oakland, which they did. Dr. Evans participated in this first conversation with Mr. Osborne and he also agreed to supply me with a technician for the sectioning and the staining of sections of the various parts of the reproductive tract of the dog and that was really his main participation in the early parts of this research. Anyhow, after we did establish--did have--the facilities built at the Humane Society it was my job to go daily to the pound--City Pound--in Oakland and select those females in estrus--early estrus--and then we had made an arrangement, of course, that we could take these animals for free, take them from the pound over to the Humane Society and test them for estrus daily by the use of some active males that we had and of course, some animals were selected in anestrus so we could follow them through anestrus into the estrous period.

Geschwind: In re-reading your Hartman Lecture, I noted with some amusement that there were some rather wild rides with Dr. Evans into Oakland in going to the pound.

Cole: Yes, this is true. Until Dr. Evans returned from Europe in the fall of 1926, I really had a very tough time getting back and forth. I used to have to get up at about 5:30 and take a streetcar from

Cole: which I had to transfer in Oakland and in the meantime I'd stop off and get my breakfast and then I'd get down to the East Oakland Pound and then over to the Humane Society facility. This trip was made by streetcar. It's about a seven mile ride on a streetcar each way. When Dr. Evans returned from Europe in September he found that we had made significant progress in delineating the changes that took place in the vagina and the uterus as well as in determining the time of ovulation in the dog and therefore he took a very keen interest. At first he would--he rode with me daily down to the pound to check on the accuracy of my description of the vaginal smear changes. We also took, of course, samples of smears for staining and also for study in Toluidin Blue in the fresh state. When he took his car, he just loved to race with the grocery boy. At that time, of course, the groceries were delivered to the home. And he also did not hesitate to run through the yellow lights whenever he got to an intersection.

But I was greatly impressed with the enthusiasm which he evinced about this work on the estrous cycle of the dog. About three years previously Dr. Evans had discovered the anterior pituitary growth hormone and was still highly involved with studies on this hormone with Dr. Simpson and with others. Also he was right in the midst of making the dramatic discovery of vitamin E, he then called vitamin X with Dr. George Burr and with Dr. Marion Goetsch. I can recall distinctly when Dr. Goetsch brought in a litter of rats probably ten to fifteen days old which had been on a low vitamin E diet and were paralyzed. He was greatly excited. Therefore, it is rather surprising that he took as much interest in my studies on the estrous cycle in the dog as he did excepting that there were certain features of this study which were of great general interest. In the first place the changes that took place in the vagina were very much like those occurring in the rat and in the mouse and the changes which took place in the uterus were much more pronounced than in any species that had been systematically studied at that time.

Geschwind: How many species had been studied?

Cole: The guinea pig by Stockard and Papanicolaou, the mouse by Edgar Allen, the rat by Long and Evans. These were really the only species which had been studied up to this moment.

Now the dog was unique as concerns the uterine changes in that the development of the uterine glands reached great complexity in post-estrus after heat, which is not characteristic of any of the other species which had been studied up to that time. The other thing which was significant was the question as to when ovulation occurred in the dog. We found that ovulation occurred either on the first or second day of the estrus. Hartman, at the Carnegie

Cole: Foundation, believed that the viability of the ovum was not longer than twenty-four hours in most species which he had studied. I think this was mainly based on the information he had with the mouse and with the opposum whereas dog breeders had contended that fertile matings occurred up to the end of estrus. And our studies indicated as I mentioned that ovulation occurred on the first or second day. That meant that the viability of the ovum in the dog must be for at least seven to nine days which is very different from other species which had been studied. The dog is somewhat uniquely different from these other species in that the second polar body is not extruded from the ovum until after ovulation occurs. At any rate, Dr. Hartman communicated to Dr. Evans that he was very suspicious of our findings that ovulation occurred on the first or second day and believed that perhaps a second ovulation occurred. To test this particular point we took two dogs and had them tested daily for estrus and then sacrificed them--allowed them to mate rather on the sixth or in the eleventh day--and then sacrificed the dogs two days later. The animal that was sacrificed on the eighth day showed male and female pronuclei in the ova which were still in the uterine end of the ova duct. In the other one sacrificed on the thirteenth day, the ova had already reached the uterus apparently. This indicated then that in the dog which had been bred on the sixth day, the ova must have had a viability of at least four days which was longer than Hartman had anticipated. This, of course, did not tell whether ovulation was spontaneous because we had tested these dogs--these female dogs--with an active male but did not allow them to mate. To check whether ovulation was spontaneous we checked three dogs from the moment of first bleeding up to the thirteenth day of bleeding and then sacrificed them and found that ovulation had occurred some time previous to this time indicating that ovulation was spontaneous in this species.

Geschwind: The project on the Estrous Cycle in the Dog was written up and appeared in the series, Memoirs University of California, and you went on then to Minnesota to take a Ph.D.

Cole: Yes. Before proceeding I would just like to say a word more about Dr. Evans. He is by far the greatest scientist I have known. At least two of his discoveries were worthy of the Nobel Prize in my view--the discovery of the anterior pituitary growth hormone and the discovery of vitamin E. Of these, vitamin E was by far the greatest accomplishment because it was literally a discovery out of the blue--that is there was no forewarning of its presence. On the other hand, it had been recognized that the pituitary had an influence on growth for many years. Shortly before the announcement of the discovery of the growth hormone, Professor Robertson in the Department of Biochemistry at Berkeley had announced in the Journal of Biological Chemistry that the feeding of anterior lobe tissue would enhance growth. This was before statistics had come into general use by

Cole: biologists and his announcement proved to be wrong and the slightly greater growth obtained in rats fed pituitary tissue was purely fortuitous. On one occasion I asked Dr. Evans if his studies had been prompted by Dr. Robertson's report and he said he was unaware of Robertson's studies until after his own work went to press. In any instance, the growth hormone is a protein and is destroyed in the digestive tract and thus is completely ineffective by mouth.

Dr. Chauncey Leake, a pharmacologist at the University of California has stated that he thought Dr. Evans had made four discoveries worthy of the Nobel Prize--he included Evans' work on vital dyes and his studies on the estrous cycle of the rat. Well, Dr. Evans was not awarded the Nobel Prize but no one can deprive him of his accomplishments which are beautifully described by E. C. Amoroso and George Corner in *Biographical Memoirs of Fellows of The Royal Society*, vol. 18, November 1972.

Geschwind: Let's get back to you. Why did you go back to the Midwest at this time to complete work?

Cole: Well, in the first place, as I have stated in my interview with Reuben Albaugh there was no opportunity to do--to take a Ph.D. in animal husbandry at the University of California. It had been ruled by the Dean of the Graduate Division, then Dr. Charles B. Lipman, that animal husbandry was not a discipline, a ruling with which I agree wholeheartedly. It's a production area that is concerned with the management of certain classes of livestock and comprises several disciplines. Therefore, the question was where I could obtain a Ph.D. working with large domestic animals. Professor Regan had been a student of Dr. C. H. Eckles, who was then chairman of the Department of Dairy Industry at the University of Minnesota and it was his suggestion that I go back and work with him. Well, Dr. Eckles had no facilities for the microscopic studies which would be essential in working on the estrous cycle of the cow and therefore he did accept me, however, with the proviso that I would do my microscopical studies with Dr. C. M. Jackson, who was the chairman of the Department of Anatomy in the Medical School on the Minneapolis campus at that time. Dr. Jackson was agreeable and this arrangement was made. Consequently, I was allowed to hold an assistantship in the Department of Dairy Husbandry on the St. Paul campus but I did my microscopic studies in conjunction with the Department of Anatomy, mainly with Dr. A. T. Rasmussen, whom you may remember did some very excellent early studies on the physiology of the pituitary.

Geschwind: Yes. And you spent two years at Minnesota?

Cole: Yes I did. I had completed most of the courses which were required while at Berkeley but I did take some extra course work in addition to the thesis project. I took some formal course work with Dr. Eckles and with Dr. Leroy Palmer and Dr. Gortner--both of the

Cole: latter were in the Department of Agricultural Chemistry. Also I took courses in physiology on the Minneapolis campus but I did not take any formal course work in the Department of Anatomy because I had some course work with Dr. Evans.

Geschwind: Did you find your stay in Minneapolis as exciting as your stay in Berkeley?

Cole: No, by no means! It was rather a--the findings that I made in regard to histological changes in the reproductive tract during the estrous cycle of the cow were rather drab. There were not any marked changes which occurred in the vagina of the cow compared to those in the other species which had been studied. The vagina showed mucification at the time of estrus but no stratification of the epithelium as in the other species which had been studied. These findings were somewhat surprising as Dr. Murphey at Iowa State College had reported that cornification of the vagina occurred at estrus in the cow much as in the rat. What's more, he showed histological figures to back up his assertion but apparently the sections were taken from the vestibule rather than from the vagina proper. Cornification of the epithelium is a constant feature of the vestibule just as it is for the epithelium of the skin. Also, the changes in the uterus were very minor as compared to those in the dog, for example, even though the cycle was intermediate in length compared to the rat in one hand which had a cycle of four to five days duration and the dog which had a cycle of six months in duration.

Geschwind: Now, perhaps I should have asked you this at the beginning but I was a bit uncertain whether one of your previous interviewers had talked to you about this. Was there some background of higher education in your family?

Cole: No, none whatsoever as concerns research. All eight children in my father's family had gone to normal school and one was a graduate of Yale.

Geschwind: What attracted you to a university in the first place, then?

Cole: Oh, this is really taken up in my interview with Dr. Heitman. But I had known since I was a small child that I wanted to go to the University of Wisconsin.

Geschwind: And what did you expect to do once you had obtained your higher degrees, go back to the farm?

Cole: Yes, I managed a 480-acre farm in Wisconsin for three years and had every intention of going back into the dairy business in California after I received my master's degree. But in the meantime, I had become interested in research and when Dr. Evans offered

Cole: me this research possibility in January 1926 I opted for it rather than going back into the dairy business.

Geschwind: Now when you finished at the University of Minnesota what were your plans--your immediate plans?

Cole: As I told in my interview with Reuben Albaugh, my plans were to return to the University of California at Davis.

EARLY RESEARCH AT UC DAVIS

Geschwind: Now you then simply packed up and left for the West again.

Cole: Yes.

Geschwind: Now I noted that perhaps to jump chronologically that in your studies on estrous cycles in mammals, you also at some later period did some work on the sheep.

Cole: Yes.

Geschwind: Can you pinpoint just when this was and what led to those studies?

Cole: This was--I began those studies about 1933 at the same time I was carrying on work on pregnant mare serum gonadotropin. The interest in the ewe was developed because the ewe is a semipolyestrous animal; in other words, it goes through a period of estrous cycles in the fall and winter and then has an anestrous period from February to September. Most black-faced breeds begin cycling about the first of September and their cycles go on at seventeen day intervals until about the first of February. In other words, the ewes come into estrus (that is they will mate) one day out of seventeen during the sexual season. Therefore, this was unique of any of the animals that had been studied at this time and I was interested in seeing if I could determine any unique characteristics of the cycle.

Geschwind: What did you find?

Cole: Well, the cycle changes in the vagina were somewhat intermediate between those which occur in the cow and those which occur in the rat or the dog. The only way we could detect estrus by vaginal smear changes was the transparent mucus which was found at the beginning of heat. You tip the ewe up on its rump, take a sample with a dental spatula, a clear mucus flows out on to the spatula which is extremely characteristic and allows one to detect estrus very accurately during the estrous season. Now prior to the sexual season and after the sexual season, the ewe goes through

Cole: a series of changes in the vagina and uterus which are comparable to those during the normal estrous cycle or during the sexual season excepting that the ewe does not come into heat. This characteristic has been referred to as silent heat which is not a good term in my view. The changes occurring in the reproductive organs during anestrus had shown no conspicuous changes or characteristics.

Geschwind: Now you had worked on the cow and the sheep in your studies in Minnesota and later in Davis. Had there been no interest anywhere else in the United States on the reproductive cycles of those large animals?

Cole: Yes. MacKenzie--Dr. Fred MacKenzie at the University of Missouri had studied the estrous cycle in the sow. Also Dr. Fred Andrews, I believe, for his Ph.D. thesis did study the estrous cycle in the mare but didn't come up with anything very exciting. The exciting part came much later as you know, when we were able to--when it was possible to follow the hormonal changes.

Geschwind: Was any work along this line being done in Great Britain? Since that time, of course, there have been lots of studies.

Cole: Yes, preceeding my work on the cow John Hammond had studied the estrous cycle and also the changes during pregnancy on the cow and wrote a book on reproduction in the cow. He also had written one on the rabbit. Both books, I think, have been pretty much lost.

Geschwind: To posterity?

Cole: Yes.

Geschwind: Harold, let's go back chronologically now. You came to Davis in 1928.

Cole: Yes.

Geschwind: And what did you get started doing here?

Cole: Well.

Geschwind: Were you involved in teaching right off the bat?

Cole: Yes, I've discussed this with Hubert Heitman.

Geschwind: How many members in the department in Berkeley had Dr. Hart brought with him to Davis?

Cole: He didn't bring anybody from Veterinary Sciences in Berkeley but he did bring Dr. Harold Goss who had just completed his thesis on calcium metabolism in the pregnant rat with Dr. C. L. A. Schmidt in the Department of Biochemistry. Actually, Dr. Goss came in 1927 to teach but didn't move to Davis until 1928.

DISCOVERY OF PREGNANT MARE SERUM GONADOTROPIN (PMSG)

Geschwind: Now your first major discovery was that of the pregnant mare serum?

Cole: Yes.

Geschwind: Which occurred shortly afterward?

Cole: Yes.

Geschwind: Would you recount what led up to that discovery? How it was made?

Cole: Yes. Well, I was--I made a report to the Journal Club of the Department of Animal Husbandry in the winter of 1928-29 on the discovery of a gonadotropin in the urine of pregnant women by Aschheim and Zondek in Germany which made it possible to determine pregnancy by biological means. Dr. Hart became very excited about this report which had been published in the Klinische Wochenschrift and it was partly my ability to read German with some facility at that time that made it possible for me to read scientific articles published in German. As a matter of fact, it was at my suggestion that Klinische Wochenschrift had been included with those journals on which I would report. He immediately became excited about this because he had been interested in the diagnosis of pregnancy in the cow. He knew that the diagnosis by palpation could only be done by an expert and therefore this idea of a hormone test for pregnancy was very intriguing to him. So he suggested that we initiate studies on the changes which occur--hormone changes which occur in the cow and in the mare. We studied those occurring in the cow and did not detect any levels of hormonal activity by the methods we were employing which was injecting the urine or the serum directly into immature rats. Our initial studies on the urine of the mare were also negative. And our first studies on the blood were negative because we were studying the blood collected fifteen to thirty days after mating which subsequently proved to be too early. But then, finally, using the immature female rat as the

Cole: test animal, we did find very high levels of gonadotropic activity in the blood of the pregnant mare which was very different from those in the human where higher levels were found in the urine than in the blood.

Geschwind: Now what did you then go on to do with this discovery?

Cole: Well, we went on to determine how long the hormone remained and we found that it was different from that of the human again in that Aschheim and Zondek had reported that there was a high rise in gonadotropic activity and then a very gradual sloping off until the end of pregnancy. They had arrived at this conclusion by injecting a constant level, that is, about five mls of urine as I recall, into immature mice. In other words, they made no attempt to determine the minimal amount of urine which would produce a response.

We, however, did determine that there was a very rapid rise in gonadotropin and that by the 150th or 175th day the hormone had entirely disappeared. We made this entire study and also determined the effect of graded levels of serum in the immature rat and made no attempt to put out a preliminary report but the report was finally made in May of 1930 in the American Journal of Physiology. This was unique at that time because practically everyone reported or at least made one preliminary report in the Proceedings of the Society of Experimental Biology and Medicine.

Geschwind: How many other species did you look at to see whether there was a pregancy gonadotropin?

Cole: Well, at that time I think it was only the cow and the horse although we may have also studied the ewe--I'm not certain about that.

Geschwind: And once the report appeared did you hear very much about it?

Cole: Oh yes. It was--people were quite, really quite excited about it. Not people in agriculture necessarily but other people were very excited. Because of the tremendous response that was obtained which was much more follicle stimulating than that obtained with human chorionic gonadotropin for example.

Geschwind: Now once having made that report, you went on to do some preliminary chemical studies on the hormone, what were these?

Cole: Yes. Well, after we had delineated the time that the hormone was present in the blood and the time it could be useful for the diagnosis of pregnancy, Dr. Hart's interest waned in the work. I think this was partly because of the fact he was chairman of the

Cole: department. He also had become interested in vitamin A levels in various feeds and natural range. He also became interested about this time in brush burning, therefore, I worked with Dr. Goss in determining the chemical characteristics of the hormone. That is, for example, we treated the serum with iodine and found that it was destroyed by iodine. We treated it with trypsin and found that trypsin destroyed it. We also--Dr. Goss also found that the hormone was precipitated with sodium sulfate as are most proteins.

Geschwind: And did you and Dr. Goss then set up a collaborative project which lasted a number of years?

Cole: Yes. We worked for at least ten years, I would presume, together rather frequently. He continued on with the studies on purification and with the assistance of a young student that had not reached the graduate status as yet, we obtained purification of the hormone of about one-half of the pure hormone. In other words, we obtained extracts which contained 7,000 i.u. per milligram. Mr. Al Booth, who later became Dr. Al Booth, with the Albany laboratory did most of this work with sera of pregnant ponies which I had shown were extremely potent as compared to those of draft horses. He should have been an author on these reports on purification studies but at the same time he was working with Dr. Goss part-time and being paid as a technician. Dr. Hart ruled that no technician could be a co-author of a paper and therefore he was denied authorship which was really too bad because he played a very important part in this study as a student receiving academic credit in A. H. 199.

Geschwind: There are a number of other people who have their names associated with work on PMS, among them Dr. Guilbert, Dr. Howell, Dr. Lyons, Dr. Catchpole and Dr. Saunders who were here.

Cole: Yes.

Geschwind: What were their contributions?

Cole: Well, we went into a series of biological studies. Dr. Hart and Professor Howell and I studied the changes which took place in the ovary of the mare during pregnancy. And we found that the ovaries became tremendously enlarged in the mare; in other words, the ovary was affected by the hormone levels--levels which are present and sometimes the ovary reached the size of a medium-sized orange. And there were a tremendous number of follicles and sometimes a tremendous number of corpora lutea indicating that superovulation had occurred and this has been since verified by other workers. I'm not too sure exactly what Professor Howell contributed to this study, that is, I made the studies of the--the anatomical

Cole: changes which were involved but at least he did go along and help us during the study collecting blood etc.

Geschwind: He was in charge of the horse herd in the department, wasn't he at the time?

Cole: Yes, he was. But all these studies were made at the Rowell Laboratory near Hayward. That is, we collected all the samples of the reproductive tracts and the samples of blood from mares that were being killed there. Now as far as Guilbert--Guilbert and Goss and myself made some early biological studies. We showed, for example, that the male accessories could be greatly stimulated by the injection of serum of pregnant mares reaching several times the mature size of the normal prostate and seminal vesicles. This is contrary to the usual concept that pregnant mare serum gonadotropin was largely follicle-stimulating but it has tremendous luteinizing activities as evidenced upon its effect on the accessory organs of the male. We later showed in conjunction with Dr. Richard Pencharz that about two rat units of the hormone would produce a response in the hypophysectomized male. Speaking of the gonadotropic units, in this same paper with Guilbert and Goss, we defined the rat unit as the minimal amount of hormone which would produce an average of three to five corpora lutea per immature female rat and this was in essence later defined as the International Unit.

Geschwind: When did you first become aware of the fact that there was another report in the literature--in the European literature--about a pregnant mare serum gonadotropin?

Cole: Well, of course, in November of this same year Zondek reported that gonadotropin was present in the blood of pregnant mares. It was rather interesting. I sent Zondek a copy of our reprint in May of 1930 and in November of 1930 he put out this report. And in the body of his manuscript he did not mention our finding but in a footnote he said it should be mentioned that Cole and Hart had made a similar finding in the United States. Now, I have a tremendous respect for Dr. Zondek but I think he was a little bit overambitious at this time and I felt he might be awarded the Nobel Prize for his finding of the hormone in the urine of pregnant women. I guess it was slightly later that he found estrogen in the urine of pregnant mares. But anyhow he was extremely aggressive and more or less shoved Aschheim out of the picture. It was very shortly after the original discovery that Zondek wrote a book in which Aschheim was not even the co-author.

Geschwind: Was there any further correspondence with Dr. Zondek?

Cole: No. Actually, I did not hold this against Dr. Zondek particularly and I had every intention of going to Germany, although I had not

Cole: written to him as yet, to work with him but along came the World War II and the preliminaries to World War II which, of course, meant that Zondek, a Jew, had to flee from Germany to Israel and therefore that sabbatical leave was aborted on my part.

Geschwind: Let's go back to my earlier question. You discuss some of your co-workers in this project. Francis Saunders' name hasn't been mentioned, however.

Cole: No. Saunders did a study--he took his Ph.D. with me and as a 199 student as an undergraduate, he determined more accurately, based on the rat unit we had previously defined, the amounts of gonadotropin which were present at various stages of pregnancy--in a more precise manner than Dr. Hart and I had done. Of course, his Ph.D. thesis later on was on the question as to whether there were one or two gonadotropins in the pituitary. We came up with the--or he came up with the concept that the pituitary contained only a single gonadotropin and he arrived at this conslusion on the basis of the following facts.

First, he had shown that he could obtain augmentation which was one of the main characteristics of the luteinizing hormone in the immature female rat, merely by taking pituitary extracts and adding a protein such as casein or egg albumen. Dr. Maxwell had previously shown that various salts would increase--would augment the response to the pituitary gonadotropin activity. Dr. Maxwell was at the Santa Barbara Hospital. And in addition to this, Saunders had tried to separate the two activities on the basis of the reports of Fevold and Hisaw and he was not successful in doing that. I don't know exactly why. But, of course, Fevold and Hisaw were not getting a clear separation either at that time. In addition to this, Saunders had worked with me and showed that the qualitative nature of the response to pregnant mare serum gonadotropin depended to a great extent upon the age of the rat. For example, if he injected an eighteen day old rat with pregnant mare serum gonadotropin, he obtained a pure follicle stimulating response. Whereas if the rat were older than twenty-one days, then one obtained not only follicular development but the development of corpora lutea as well. And this he thought provided evidence for the one hormone concept. Now, of course, it was later clearly demonstrated that the pituitary contained two gonadotropins, follicle stimulating hormone and luteinizing hormone although there still is a lingering doubt in the minds of certain people concerning this. For example, McShan and Sherwood have shown--have had extracts which are more potent than most so-called pure hormones which had both follicle stimulating and luteinizing activities. I don't have a very good interpretation of this particular finding.

Geschwind: Well, this is something that Meyer's group has pushed for some time, I guess. What about the contributions of Bill Lyons and Hubert Catchpole with pregnant mare serum?

Cole: Yes, I will but before getting into that I would like to mention the study of John Santolucito which he did jointly with Dr. M. T. Clegg and me on the FSH and LH content of the pituitary in ewes at different stages of the estrous cycle. He found a precipitous drop in both hormones at the beginning of estrus which suggested that both hormones were released at the same time. This preceded by about ten years the discovery by Schally and his associates at Tulane University of a hormone produced by the hypothalamus which would cause the release of both FSH and LH. Incidentally, Dr. Santolucito has spent most of his time after completion of his Ph.D. studies working on nerve physiology in the National Institute of Health rather than in the area of his special training in the field of endocrinology. This illustrates the importance of a broad basic training in physiology.

Now I will return to your question concerning Hubert Catchpole. He worked with Lyons and I together as co-directors of his research activities for his Ph.D.

Geschwind: Roughly what year was that?

Cole: He got his degree, I believe, in 1933. And he became interested in the gonadotropic activity of the placenta and, of course, he also studied the blood and the placenta simultaneously. And he came up with the view that the hormone was actually produced by the chorion of the pregnant mare and, of course, this fits in with the latest finding of W. R. Allen at Cambridge, in which Allen has shown that he can take the chorionic tissue in the so-called girdle area and culture it and show that it produces tremendous amounts of gonadotropin. But I am of the feeling that the first studies by Catchpole and Lyons, in which they show a tremendous amount of gonadotrophic activity of the chorion was due to adhering endometrial cup secretion rather than to any activity of the chorion because in later studies which Goss and I made we found if we carefully washed the chorion free of endometrial cup secretion that there was no activity--no detectable activity in it. But anyhow his interpretation later proved to be correct even though I don't think his basis for making this prediction was very accurate at that time.

During the same time both Dr. Hart and myself found that the fetal gonads had become tremendously large about the 200th day of pregnancy and Lyons and Catchpole had made a similar finding. Well, when we found out that we were both about to write the same paper, we decided to collaborate and put out a single paper which

Cole: we did. So some of the figures which are in that particular paper in the Memoirs University of California were prepared by Catchpole and Lyons and some of them by Dr. Hart and myself.

Geschwind: Catchpole also worked with you on the source of estrogen in the pregnant mare.

Cole: Yes. Perhaps before I get into that I should mention one other study that Catchpole made. After he got his degree, he took a position at the Kellogg Institute which is near Pomona with the Department of Animal Husbandry and during that time one of the important contributions he made was on the half-life of pregnant mare serum gonadotropin in which he injected with our help tremendous amounts of gonadotropin--I think it was 25 or 50,000 international units. Not a tremendous amount, really, but a fairly large dose. He injected this into geldings. He also injected it into rabbits. He found that the half-life in the rabbit was approximately twenty-four hours whereas in the gelding it was approximately six days. Now, of course, in the gelding the testes are absent so very much later in about 1970, I again took up this question to determine if the half-life would be the same if the gonads were intact. This work was done cooperatively with Mary Bigelow Horton and two young veterinarians on a postdoctoral year with Dr. Hughes in Veterinary Clinical Medicine. Mary was serving as my technician and at the same time she was working toward her M.S. degree with me. Therefore, we took a pregnant mare and hysterectomized her in early pregnancy at the time of the peak of the gonadotropin and we showed that in the hysterectomized mare in which the ovaries were intact the half-life was also six days. Therefore, it appears that the gonads do not play any significant role in the metabolism of the gonadotropin in the horse.

Geschwind: And the PMSG has a fantastically long half-life.

Cole: Yes much longer than . . . well the only gonadotropin that comes any where near approaching it is HCG. But to get back to your question about the studies on the source of estrogen, Catchpole studied the excretion of estrogen in the urine of the mare before and after parturition and in the first voided urine and later urines of the foal. He showed that hormone was present before parturition but by twenty-four or forty-eight hours after parturition the estrogen was completely absent by his method of testing in the urine of the mare and was also absent in the second voided urine of the foal. This indicated then that some gland had been eliminated at the time of parturition and the one gland which one immediately thinks of is the fetal placenta. This indicated that the placenta was involved rather than the ovaries. Dr. Hart and I followed this up with a study later on in which we ovarectomized a mare during pregnancy and showed that the level of estrogen in the urine was

Cole: essentially the same as in the intact animal indicating the ovary played no significant role in the secretion of estrogen during pregnancy.

Geschwind: Now, this pretty well completes the story on discovery and subsequent experiments with pregnant mare serum. You also collaborated with Professor Hart on studies on growth during much of that time.

Cole: Yes. There is one other subject that I would like to mention particularly and that is the source of pregnant mare serum gonadotropin. I've alluded to this very briefly but in 1943 Dr. Goss and I published a paper in Essays in Honor of H. M. Evans published by the UC Press in which we indicated that PMSG was produced by special structures in the endometrium which we referred to as endometrial cups, because they were more or less cup-shaped and we showed that these endometrial cups contained tremendous amounts of gonadotropin as did the secretion which came from these cups. As a matter of fact, the activity in the endometrial cups secretion came as high as 300 i.u. of PMSG per milligram of fresh tissue which represented a tremendous amount compared to that in the blood of the pregnant mare. Well, we postulated then, that the endometrial cups secreted the hormone. Of course, as I've indicated, W. R. Allen later showed that if he took the girdle--the chorionic cells from the girdle area--and cultured them--that they produced tremendous amounts of hormone and furthermore that they took on the characteristics of the decidual-like cells in between the uterine glands. He also, of course, had shown the migration of the chorionic cells into the endometrium. Therefore, I think he has conclusively demonstrated that the chorionic girdle cells are actually the source of the gonadotropic activity although there are several things which are rather mystifying. One of these is that if you follow the study of the endometrial cups from the point of their genesis, the first change that you note is a widening of the lumens of the uterine glands in the cup area. That's the first thing that you notice. There are no decidual-like cells present at this time when the hormone is first being secreted. This is rather mystifying but at least it demonstrates that you do not have to have the characteristic decidual-like, the tremendously larger cells, in order to have gonadotropin secreted.

Another point that is rather interesting is how does the endometrium--how does the pregnant mare serum gonadotropin get from these decidual-like cells into the lumen of the uterine glands. It apparently somehow migrates to the uterine gland and from the uterine gland it's secreted along with other uterine gland secretions such as mucus into the space between the chorion and the endometrium. And since we're dealing with a protein it's rather surprising that it could pass in such an astonishingly large amount from the connective tissue area into the uterine glands.

Cole: I have mentioned Mary Bigelow Horton's collaboration on the study of the half-life of PMSG. I would like to mention one other joint study we made on the question as to whether human chorionic gonadotropin (HCG) had intrinsic follicle-stimulating activity. Crook and Butt, in England, had shown that the urine of pregnant women contained some pituitary FSH but the question was whether the small amount of contaminating FSH in human chorionic gonadotopic extracts obtained from the urine of pregnant women would account for the follicular response when large doses of HCG were given. By injecting serum containing HCG antibodies in conjunction with HCG, we found that these antibodies would inhibit the follicular response as one might expect if this response depended solely upon HCG. Thus, we concluded, as did Dr. Albert of the Mayo Foundation using a different approach, that the HCG molecule has intrinsic follicle-stimulating properties.

I should also mention that John Santolucito obtained his Ph.D. with me working on the levels of follicle stimulating and lutenizing hormones in the pituitary gland at different stages of the estrous cycle in the ewe. He found a sharp drop in the level of these hormones occurring between the fourth and thirty-fifth hours after the beginning of estrus--a finding consonant with more recent studies on blood levels.

John Nellor studied the control of ovarian activity by progesterone in the beef heifer for his Ph.D. thesis. John has recently been made Dean of Graduate Studies at the University of Nevada, Reno.

Rather reluctantly, I will mention that Drs. Hart, Goss and myself obtained two patents in 1935 based on our discovery and purification of PMSG. These patents have made possible the establishment of two graduate scholarships in Animal Husbandry which were valuable in helping finance graduate students while taking necessary course work in Berkeley. Thus, from the standpoint of the department they were valuable but at the same time I consider this action as somewhat of a blight on my academic career.

A study by John Erway for his M.S. degree resulted in a 48-hour method of assay of PMSG. This study was published in Endocrinology and has become a widely used method of bio-assay for this hormone.

Another M.S. student, Paul Hinshelwood, deserves special mention. Although his research was of little consequence, his graduate training has led to an important position with Los Angeles County in control of alcoholism.

STUDIES ON GROWTH

Geschwind: Perhaps we can return now to the question about the simultaneous studies on growth which you had been carrying out with Dr. Hart.

Cole: Yes. I became interested in this particular study on the basis of earlier work on superfecundity induced in the immature female rats by the injection of gonadotropin. I had shown that if we injected about ten units--ten international units of pregnant mare serum gonadotropin that the rats would become pregnant following mating if they were injected on about the twenty-eighth day (twenty-sixth to twenty-eighty day) and that they would go through a normal pregnancy. And in fact instead of being stunted by this procedure which was a common belief at the time they actually were somewhat larger than normal.

This prompted us to study the effect of pregnancy on growth in the rat. And, therefore, we had rats--we injected rats with gonadotropin so that they became precociously mature and then as soon as their first litter was weaned they were bred again. We found that rats that went through nine pregnancies were actually about 25 per cent heavier than litter mate controls that were kept as virgins. This was a true growth as indicated by increase in the tail--nose to tail length of these animals. It was not just an increase in body weight. We were interested in determining whether this was an effect of the fetus--some substance coming from the fetus or whether it was due to some substance produced by the mother itself. Therefore, we allowed rats to be mated to vasectomized males and found that if they were mated to vasectomized males that every time they went through a pseudo pregnancy they grew at almost the same rate as the pregnant rats, indicating that it is some substance derived from the mother itself which is responsible for this increased growth.

This is an interesting finding which has never been developed further. Contopoulos--Dr. Contopoulos and Dr. Miriam Simpson on the Berkeley campus did put out one report indicating that if the blood of pregnant rats were injected that they would grow faster

Cole: than if they received blood from non-pregnant rats. And that's where the matter stands. We were interested in this question from a more practical standpoint also in determining whether we could get a faster rate of growth by breeding cattle prior to selling. So we studied the effect of pregnancy on the rate of growth in beef heifers. However, we found that in beef heifers there was no stimulation to growth as in the rat. And later I studied the same thing in sows and found the same to be true here. Therefore, there is no practical application of this finding in livestock production.

Geschwind: Well, I think that pretty well completes the studies on growth and we can now turn our attention to studies on physiology of the rumen as related to the problem of bloat in the cattle.

Cole: Yes. As I mentioned earlier, Dr. Hart had introduced a rumen fistula into a cow very shortly after I arrived on the Davis campus and consequently I had been working with fistulated animals off and on throughout this time and had a number of 199 students studying characteristics of rumen contraction. Well, at about the time World War II started the department became rather financially embarrassed and the question was how do we cut down on the amount of funds that were being utilized at that time. Dr. Hart decided that we should maintain the herds, specifically the inbred Jersey and Holstein herds because they were long-time studies which could not be taken up again. And therefore the activities in the rat colony were greatly diminished. We were practically devoid of activity there for several months. I actually took care of the rats myself.

THE QUESTION OF BLOAT

Cole: Well, this meant that the studies we had been making on the physiology and the chemistry of gonadotropins had to be terminated because we had to use fairly large numbers of immature rats for assay. Also at about this same time Dr. Hart had decided that it was wrong for people in the scientific group which at that time according to his classification consisted of Max Kleiber, Paul Gregory, Harold Goss and myself and that we should not be collaborating with each other but rather that we should collaborate with members of the so-called production group. Anyhow, because of the fact that I did have cows available but not rats I did become interested in this question of bloat. Of course, there were many reports of bloat, serious bloat problems in the field and therefore the first study was undertaken with Professor Mead, Dr. Kleiber and myself. The first thing that we studied was whether there was a difference in either the amount or qualitative nature of the gas which was produced on bloat-provoking and non-bloat provoking diets. And we showed that on bloat-provoking and non-bloat provoking diets a tremendous amount of gas was produced immediately after feeding. It amounted to about a cubic foot of gas per hour. Therefore there was not a difference in the amount of gas or in the qualitative nature of the gas. We found the gas contained about 65 per cent CO_2 and about 30 per cent methane, a little bit of hydrogen and of course some oxygen which was not necessarily a metabolic product. Therefore, we postulated that bloat was due not to the amount of gas produced or the type of gas produced but rather to the inability of the animal to get rid of the gas and we further postulated on the basis of information we had available at the time that bloat was produced because of a lack of eructation, because of a lack of stimulus of scratchy material on the rumen wall.

Geschwind: Who . . . let me interrupt here--who had done work in establishing the natures of bloat-provoking diets?

Cole: Really there had not been any field work to my knowledge on this point with one or two exceptions. Professor S. W. Mead had earlier

Cole: shown that he could raise heifers on a roughage-free ration and one of the characteristics of these animals was that bloat regularly occurred but not of a serious nature. Other than this it was mainly field observations that if animals received hay they were less likely to bloat than if they were fed succulent legumes.

Geschwind: So for your experiments you used bloat-provoking diets and then you had some very definite type of feed in mind?

Cole: Yes, we had green alfalfa in mind because this had been shown previously to produce bloat. And then our next step was to show that we could effectively prevent bloat if before pasturing on alfalfa the cows were fed a diet of Sudan hay. Sudan hay, as you know, is unique in that it has--that the leaves are barbed and therefore, very scabrous in nature. And we found that if the cows were fed ten or fifteen pounds of certain hays preceding pasturing that bloat was effectively prevented. We also showed at this time that it was not a question of the amount of alfalfa that they consumed, that is, one might suspect that if they are fed ten or fifteen pounds of hay that they would consume less alfalfa but this was not the case. They would actually eat more alfalfa in most cases. When an animal begins to bloat the tendency is to reduce--stop eating.

Geschwind: You also did some work on foaming--an anti-foaming agent?

Cole: Yes. In conjunction with Dr. James Boda we did this work in which it was shown that if a large amount of egg albumen was fed that foaming was produced and the cow would bloat on diets that would otherwise not produce bloat. And later on Dr. Boda carried on studies with vegetable and animal oils and showed that if a certain amount of these oils were sprinkled on fresh legumes that bloat was not produced. But actually the original finding on it as far as animal fats were concerned was made by Dr. Johns in New Zealand but Dr. Boda carried on more experiments along this line showing the exact amounts that were necessary to add to a certain amount of hay--to green alfalfa to prevent bloat. Dr. Johns' work had all been on ladino pastures in New Zealand.

Geschwind: Well, before we turn to another subject is there anything more you would care to say about the bloat story? Where do you feel it is today?

Cole: It's--there have been other--well, there is perhaps one other thing. We were not--in our earlier studies--we did not think foaming was of any material account because of the fact that we could introduce a trocar cannula and relieve the gas in the cow whereas we assumed if foaming occurred this would not be possible. Well, it so happens that there is always a gas pocket at the dorsal

Cole: portion of the rumen. But we were finally able to allow one of these cows to go to the point of death from bloat and when we did this and opened up the rumen we found that there was a tremendous amount of foaming. And therefore, foaming was an important factor and we believe--well, we're still not certain exactly why the animal does not eructate. As I mentioned earlier, we felt that there was not sufficient scabrous material to induce the reflex act of eructation. But it's more likely, I presume that if the obstruction of the esophageal orifice by foaming prevents eructation from occurring, the lack of scabrous material per se may influence the amount of gas released at each eructation. This is indicated by studies made by Dr. Harry Colvin as a graduate student. He devised a refined means of measuring the amount of gas formed, and more importantly, the amount of gas expelled at each eructation. His studies indicated that scabrous matter in the diet influenced, not the frequency of eructation, but the amount of gas expelled at each eructation. Further refinements of these findings were made by the studies of Dr. Verne Mendel in his graduate studies under the direction of Jim Boda which indicated that scabrous materials may enhance salivary secretion and thus inhibit foaming.

Geschwind: Well, to summarize much of this, I suppose we can say that it took the world war to force you away from science and into the practical application of science (laughter).

Cole: That's right (laughter) I'm sure I would have never gotten into it any other way because I was very intrigued with the things that I had been doing.

RESEARCH ON EFFECT OF CALCIUM ON REPRODUCTION

Geschwind: You had mentioned earlier that back in your Minnesota days you had looked at calcium and its effect on reproduction. I suppose it was somewhere in the late 1940s or early 1950s that you turned your attention to calcium again but this time in terms of practical research of another disorder of cattle. Would you talk about this now?

Cole: Yes. When I was teaching systemic physiology, I ran into an article by Dr. Forbes--I don't remember his initials, of Yale University, in which he'd indicated that if you fed a low calcium diet to rats that there was an enlargement of the parathyroids and an indication of an increase of secretory activity of parathyroid hormone both in the rat and the rabbit. Thus I suggested to Dr. James Boda that he might take up the effect of low calcium diets on the incidence of milk fever in cattle, and he did this for his thesis. He demonstrated that if cows were placed on a very low calcium diet which meant, of course, no legumes and a high phosphorous diet for about six weeks preceding parturition that milk fever was effectively prevented. Of course, in order to do this particular experiment it was necessary to work with cows which had had previous case histories of milk fever both for controls and for the experimental animals. In order to provide a low calcium diet, it was necessary to feed a very high level of concentrates which were low in calcium and high in phosphorous and a roughage such as Sudan hay which is relatively low in calcium. Our postulate then was that a low calcium diet increased the secretory activity of the parathyroids and thus maintained blood calcium levels in early lactation.

In other words, at the time that lactation begins right after parturition there is a tremendous change in the calcium metabolism of the cow because of the tremendous amount of calcium which is secreted in the milk. Thus we postulated that by placing the animals on a low calcium diet ahead of time we would increase the secretory activity of the parathyroid glands early in lactation and thus prevent the incidence of milk fever. As a matter of fact, Cole: this particular theory has never been completely proved because some studies, I believe, that were done by somebody at NIH, I can't think of his name but who has also been interested in this general subject--his name escapes me at the moment.

Geschwind: Potts?

Cole: Yes. John Potts and his associates in cooperation with somebody at Pennsylvania State University or with the University of Pennsylvania rather; I believe that they did not substantiate this particular viewpoint. In other words, they did not find any increased secretory activity of the parathyroids on these low calcium diets. How then this particular regime is effective in preventing milk fever is still not clearly understood. I suppose it's possible that it may be associated with the calcitonin levels rather than parathyroid hormone but I'm not sure.

Geschwind: I gather that this feeding animals diets that were low in calcium and high in phospherous beforehand has not been widely utilized as normal methods in which one administers vitamin D in large quantities to the animals and so on.

Cole: I think. I talked with Clem Pelissier about this particular point because he is in close contact with the dairymen, of course, being extension dairyman. And he thinks that actually there is considerable use of high concentrate feeding prior to parturition which would tend to carry out the proposal which we had made. But this is true, in general--generally speaking--livestock producers love to inject something rather than to change the management practices and I am sure that there has not been as much use of this finding. Of course, another inhibiting factor is that the supply of hays which are low in calcium are relatively small; there is a limited supply of Sudan or oat hay produced in California. This is one of the limiting factors, there is no doubt about it.

A MATTER OF GONADOTROPINS

Geschwind: Well, now let's shift to the last of the major areas of research interests. When I first came to the department and in the time that I've been here, you've been interested almost solely in matters of gonadotropins. How was this interest re-established?

Cole: Yes. I originally became interested in it because of the result of studies in which estrogens and progesterones had been used to synchronize estrus. They were not too successful. In other words, although one could synchronize estrus the number of pregnancies resulting following mating in this first estrus after estrogen administration was very low. In other words, the treatment with estrogen and/or progesterone was somehow inhibiting ovulation presumably. Therefore, I thought because of previous studies which had been made by Idwal Rowlands in Cambridge and Kenneth Thompson in this country and others in which they had shown that if the gonadotropins were given as antigens that an antibody could be formed against these gonadotropins and I felt that the use of antigonadotropins would be a more natural way of synchronizing estrus in domestic animals. Consequently about this same time a student came along, Robert Snook, and he became involved with these studies. So we started injecting various gonadotropins. We injected LH, FSH and HCG but we soon found that it was very difficult to develop high titers of gonadotropins against pituitary gonadotropins such as LH or FSH whereas it was relatively simple to get high titers of antibodies against human chorionic gonadotropin and therefore most of our studies have depended upon HCG. Now, however, the antibodies against HCG are not very effective against pituitary, LH or FSH. As a matter of fact, we found that if antisera against HCG were injected in conjunction with pituitary FSH or LH we actually got an augmentation rather than an inhibition of the response. And therefore, most of our concerns since that time have not been with the synchronization of estrus but in the studying of this augmentation response to antigonadotropic sera.

We found that if the antiserum is given alone to immature female rats that there is an ovarian and uterine development in

Cole: these animals and we later found that if we injected this antiserum into hypophysectomized male rats that there is a marked response in the prostate and seminal vesicles, indicating that there is definitely gonadotropic activity present in these antisera and that it is not really an indirect response. However, if one assays the amount of gonadotropin by either the ascorbic acid depletion assay or by radioimmunoassay, the amount of gonadotropin is too small to account for the gonadotropic responses which were obtained. Thus, we tentatively decided that the endogenous gonadotropin present in the antiserum is actually complexed with HCG antibodies to produce a larger aggregate and thereby enhancing the response of the gonadotropin. Some questions from our studies are: why does HCG antiserum produce a gonadotropic response in hypophysectomized male rats but not in hypophysectomized females? If we use HCG antiserum with FSH activity we are going to inhibit the FSH response and therefore we would not get a response with LH alone in the hypophysectomized female. And that would explain why we do not get a response in the hypophysectomized female because it does not give a response to LH alone whereas the hypophysectomized male does.

Now another question which arises, is the progonadotropic response due to a complexing of HCG antibodies with gonadotropins. If this were true, if it actually were a complexing with HCG antibodies then one should be able to obtain a gonadotropic response or a progonadotropic response with every HCG antiserum but this is not true. We have some HCG antisera which are very high in antibody activity but with which we cannot obtain either a progonadotropic response or a gonadotropic response.

I should perhaps deviate for a moment to explain what is meant--what we mean by the progonadotropic response. If HCG antisera are injected in conjunction with pituitary FSH or LH, the response of either of these gonadotropins is augmented. In other words, this is what would be referred to as a progonadotropic response, an augmentation of the response of the gonadotropin given concurrently.

The second piece of evidence is that the progonadotropic activity and the antigonadotropic activity can be separated by affinity chromotography in which HCG antibodies are bound to the HCG on the sephadex column whereas the probodies pass freely through the column. Therefore, this indicates that HCG antiserum contains two types of antibodies, the neutralizing antibodies and the non-neutralizing antibodies. It is the non-neutralizing antibodies that presumably complex with pituitary gonadotropin to give either the gonadotropic or progonaotropic response.

Now another question which arises is that if we give HCG in conjunction with HCG antiserum plus pituitary FSH for example, the

Cole: progonadotrophic response is inhibited. Why does HCG do this? One explanation might be that the nonneutralizing antibodies are attracted more to HCG than they are to the FSH and thus they do not tie up the FSH and increase the size of the molecule and increase the progonadotrophic activity. However, this has not been shown. Would you have any ideas on this? Of course, you have been very intimately involved with all of this research.

Geschwind: Well, I have ideas but I think this is your day (laughter).

Cole: (Laughter.)

Geschwind: Where do you see this work leading?

Cole: We've gone up a blind alley as far as using HCG antibodies to synchronize estrus in large domestic animals. Simply because instead of getting an antigonadotrophic response we get a progonadotrophic response. From a practical standpoint, this may be of interest in increasing the physiological activity of FSH and LH extracts. In other words, you can get a gonadotropic response with a much smaller amount of hormone when you give it in conjunction with HCG antiserum. There is another antigonadotropic activity which I think has some application. We find that the HCG antiserum will inhibit human pituitary FSH and LH, both pituitary FSH and LH in about the same degree and thus it is entirely possible that we could use the HCG antiserum in controlling reproductive activity in the human female. In other words, as a means of preventing ovulation from occurring, for example. This may be practical because of the fact as I mentioned, it's very difficult to get high antibody titers against either pituitary FSH or LH whereas it's easy to get high--relatively high antibody titers against HCG. So therefore although we were unsuccessful in the original objective, we have found, I think, some properties of HCG antiserum which may be of practical significance either in controlling reproductive activity in the human female or in increasing the activity of FSH and LH in domestic animal species in the therapeutic use of these hormones in treating reproductive difficulties.

Studies of two postodoctoral fellows, Drs. Cornelius Ahern from the School of Veterinary Medicine, University of Dublin and Lubomir Itze from the School of Veterinary Medicine in Krakow, Poland, while working jointly with you and myself, were especially fruitful in providing a basis for our current concepts of the progonadotropic response to HCG antisera.

SABBATICALS

Geschwind: All right now I think we've come to the end of the major studies. One question which I thought would come out in the course of discussion on the antigonadotropins but did not had to do with the sabbatical leave you had in Denmark. We've been talking about research which had been carried out during your graduate career and then here at Davis. You've taken a number of sabbatical leaves, I suppose, in your academic career. I know of the one in Denmark. Where did you go and what did you accomplish on these leaves?

Cole: Actually, I've only had two sabbatical leaves. The first sabbatical leave was in 1936 when I spent three months at Beltsville in which I attempted to determine the levels of gonadotropin in the urine of pregnant cows, which was not very successful because presently available sophisticated procedures for determining levels of gonadotropins were not available at that time. From there I went up to Yale University for three months to work on the effect of gonadotropin in rhesus monkeys with Dr. Gertrude VanWagenen. Because of some studies which I had done previously I thought that the only reason they were not able to control--to get a normal physiological response in the rhesus monkey was because of the quantitative factor. In other words, as I indicated to you before that if you give very large amounts of--no perhaps I didn't indicate to you before--but if you give very large amounts of a gonadotropin you get an abnormal type of gonadotropic response in the rat whereas, if you give minimal levels you get a normal physiological response if the immature rat is injected somewhere between the twenty-sixth and twenty-eighth day of age. I thought probably the problem of producing ovulation in the rhesus monkey was dependent upon this quantitative factor in which exceptionally large amounts of hormone had been given and therefore if we got down to minimal levels a normal physiological response could be obtained.

However, this did not prove to be correct. We did show that we could get a beautiful gonadotropic response, that is we could greatly increase the size of the ovary of the macacus rhesus

Cole: monkey but we could not produce ovulation at will. Of course, later, Dr. VanWagenen in conjunction with Dr. Miriam Simpson showed that when they used primate gonadotropin they could produce a normal physiological response.

In 1956 and '57 I spent a year working with Dr. Christian Hamburger at the Staten Serum Institute in Copenhagen, and with Dr. Johannes Moustgaard at the Agricultural College in Copenhagen working on the induction of antigonadotropic activity in cattle. This was really a prelude to the studies which were done later but I used pregnant mare serum gonadotropin and although I could get high pregnant mare serum antigonadotropic activity, this antiserum was not effective in controlling reproductive activity in cows. In fact, I showed actually that the ovaries in the injected heifers actually increased in size rather than diminished in size even though there was high anti-PMSG activity.

Geschwind: You did feel that the year spent in Copenhagen was well spent, however?

Cole: Oh yes, of course, it was a very fine year, first with my contact with Dr. Christian Hamburger and then with Dr. Moustgaard and with many other contacts that were made during the year.

PHILOSOPHICAL OBSERVATIONS ON CHANGING DEPARTMENTAL RESEARCH

Geschwind: All right you came to the department in 1928. You officially retired in what year?

Cole: 1964.

Geschwind: All right so that was thirty-six years of active work in the department and then you entered a very active emeritus status which has been going on now for approximately twelve years. This is almost half a century of close attachment to this department. Would you look back now and tell us about the type of research you first saw going on in this department and how it's changed in nature and then give us some idea, perhaps in greater detail, as to the type of research that was going on during the period in which you were chairman of the department?

Cole: Yes, I would be happy to comment on that. I first arrived in Davis as a graduate student in 1924, or at least that was my first contact on campus, although I wasn't here permanently, rather I was in Berkeley. There really was no significant research under way at that time. Most of it could be described as demonstration research. They showed that they could feed certain diets to cattle and that animals grew and fattened to be sold. The major part of the activity of the department was concerned with non-degree teaching, that is the so-called non-degree program and with extension. And the department members spent a lot of their time going out into the field, attending livestock meetings of various sorts, and judging at livestock exhibitions and this sort of thing. Of course, there was at that time only one livestock extension specialist and he was stationed at Berkeley. Therefore, perhaps more attention to the extension activities was very desirable at this time.

With the advent of Dr. Hart in 1926, the nature of the research activities took a strong turn. At this time Dr. Hart was a firm believer that research should be based on scientific principles and he divided the department up into two groups. The

Cole: newcomers Goss, Kleiber, Dr. Gregory and myself were called the science group. The rest of the people were production people. He thought that there should be a strict division of the activities of these two groups--that the production people should be the ones that had contact with the field. As a matter of fact, I did not get out to the field to attend a livestock meeting until about fourteen years after I came here. And consequently the so-called production group had almost complete contact with the livestock producers. Dr. Hart, as I say, strongly believed in scientific research and did a great deal to start basic programs. He did not believe, I think, as evidenced by his own activities for the most part, that people in animal husbandry should be concerned primarily with basic research. He felt that we should be primarily concerned with applied research although he did realize when he got here that before you could do applied research there had to be a certain amount of basic science research background. And we were not going to find this type of background in many studies, for example, the bloat studies that I mentioned. You were not going to get the background research from the departments of physiology for example in medical schools because they were not involved in the physiology of domestic animals. More recently they are more inclined to do research on large domestic animals but earlier they were not.

Geschwind: Let me interrupt to ask you some questions about this. Dr. Hart was chairman of the department at the time and I suppose continued as chairman for another twenty years when he went over to the veterinary school. He actively recruited the scientific crew and brought Max Kleiber over from Switzerland in that group.

Cole: Yes.

Geschwind: Would you care to say something about Max Kleiber's efforts in that early department?

Cole: Yes. His early efforts, of course, were largely involved in the developing of the respiration chamber and adapting it for research in the metabolism of cattle. I presume he must have spent at least fifteen years in which he was changing the chamber and there were two or three articles, I believe, on research using this chamber. One was related to the influence of phosphorous on metabolic activity of the dairy cow and perhaps one or two others. He had one large Hilgardia article which described in detail the respiration chamber he developed, which is the open chamber as compared with the closed chamber which had been previously used, I believe, by Armsby at the University of Pennsylvania. Then, of course, he did collaborate with many other people in the department on other projects. He worked with me and with others on the influence of hormones on metabolic activity. And it was largely, I think, the impact of his philosophy in regard to the three-fourths power of

Cole: the body weight and metabolic unit that was a very important factor as far as departmental activities were concerned. Up until this time, people in the department that were interested in nutrition, for example, were largely basing their results on body weight and he played an important part in changing this particular concept. Not only as far as the body size is concerned but in the nature of the increments in weight. That is, were they fat or were they protein? I think it was his thinking that made an important impact on Lofgreen and Meyer who later made some very important studies relating to the nature of body gain and developed the concept that you could determine the nature of the gain simply by weighing the carcass in the air and in water.

Of course, it was not until the late forties when Dr. Kleiber went back to the University of Chicago that he became interested in radioactive isotopes and from that point on he made many very important discoveries related to intermediate metabolism in the dairy cow, particularly as it related to the precursors of milk constituents, for example. He worked very closely with Dr. Arthur Black and Dr. Jerry Kaneko and the School of Veterinary Medicine and carried on a very fruitful period of research for a long period of time.

Now as far as the practical aspects, he never did get into a great deal of that except it was his philosophy that was important as far as the practical aspects were concerned. He did, of course, work with me on the nature and the amounts of gases which were produced in bloat and non-bloat provoking diets as I brought out previously, which was, of course, really a basic problem but which had practical applications. In addition, to the studies which he had made as I mentioned, Drs. Lofgreen and Meyer initiated some very important applied nutritional studies and some which had more basic significance. But most of them were applied as related to the utilization of pastures and determining the nutritive value of hays. They showed that they could determine the nutritive value of alfalfa, for example, by determining the crude fiber content of the hay.

Then during this period Dr. Gregory was involved with a series of experiments relating to growth. He had become interested in growth very early in which he studied the rate of growth in small and large rabbits. And he initiated a series of studies in which he studied the rate of cell division and showed that there is a greater rate of mitotic divisions very early in the embryo of fast growing large rabbits as compared with smaller ones. And this led him to his studies on glutathione with Dr. Harold Goss which I don't think had any great significance but they--he did--he was able to show a relationship between the glutathione content and the rate of growth of animals. He also worked with Dr. Admundson in poultry in this connection.

Geschwind: Harold Goss, who you previously mentioned in terms of his work with you on the pregnant mare serum gonadotropin, here we have him working with Dr. Gregory in terms of the glutathione studies. He also later on did some work on electron transfer mechanisms.

Cole: Yes.

Geschwind: And he was very active in setting up biochemistry in this department. About what time was that?

Cole: Yes. He came about the same time I did, whether it was the same year or the next year I can't be sure, but he taught at least one year before I arrived. But I think that Dr. Goss' main contribution was his very excellent work on calcium and phosphorous metabolism. Of course, some of this work was in conjunction with Professor Harold Guilbert, and as a matter of fact he worked on calcium metabolism for his thesis on the rat. But as far as his applied studies were concerned, perhaps his most important contributions were related to his studies on the molybdenum toxicity in certain grasses in the San Joaquin Valley which were very high in molybdenum to the point that they were toxic. And he showed that this toxicity could be overcome by giving--well apparently the molybdenum was interfering with normal copper metabolism and by giving injections of copper he could overcome these toxic effects.

Another investigator that made some important contributions, of course, was Professor Harold Guilbert on his vitamin A studies. Not only did he show differences in the vitamin A levels in different stages of maturity, he actually determined the minimal requirements for vitamin A in various classes of livestock.

Geschwind: Aside from these studies on pregnant mare serum gonadotropin, what were Professor Hart's interests in research? Did he have much time for research?

Cole: Well, he collaborated with various people. As I've indicated he worked with Professor Guilbert on vitamin A studies and collaborated with him. He was not able to put in a lot of time because he had administrative duties but he did--I'll say one thing for Dr. Hart, he was a very inspiring influence. He became exceedingly excited about any new research ventures and I'm sure if it hadn't been for him, I would not have been able to get started on the pregnant mare serum gonadotropin as rapidly as I did. He served as a very important catalyst in getting this work under way. After he reached the objective which was, for example, in the pregnant mare serum gonadotropin, a means of determining pregnancy in the mare, he more or less lost interest. He did not see beyond that there might be important uses of PMSG from a practical standpoint. He was more interested in applied research. But he had some characteristics which impeded

Cole: I think the progress of the department in that he never could get away from the concept that there should be two groups of researchers, scientists and production men. Now as you know, before I became chairman the change in department philosophy had already taken place and he had contributed to this to some extent by bringing in people such as Lofgreen, Meyer, Heitman and Floyd Carroll. Floyd Carroll, incidentally, took his graduate work with Dr. Goss and made some important contributions on synthesis of B vitamins in the cecum of the horse. And Dr. Goss had previously worked on the synthesis of B vitamins in the rumen of the cow with another graduate student, Dr. McElroy now in Manitoba.

Geschwind: Well, what I would like to do is to return to the early scholars in this department about which there is probably very little in the way of an oral history. Can you tell us more about Professor Gregory, Professor Mead, Professor Howell, Professor Wilson?

Cole: Yes I can . . . I've discussed with Hubert Heitman Professor Regan's work on inbreeding in Jerseys. This was his main research activity throughout his life. Although he participated in some other research this was his main emphasis. With him came Professor S. W. Mead. And he acted more or less as his helper throughout most of his career although Mead did do some original studies as I mentioned earlier on concentrate rations in dairy cattle showing that roughage was not necessary, although there were certain problems such as chronic bloat. Beyond the uncovering of many deleterious recessive autosomal genes which I referred to earlier an important contribution, I think, that Professor Regan made was that he could carry on this inbreeding line for a long time without any severe adverse affects in the line of Jerseys he used. He ran into an entirely different problem in the Holsteins and so I presume it depended entirely upon the genetic make-up of the starting material because the Holsteins completely degenerated as far as milk production was concerned. Some of them didn't even give enough to milk more than a month or so. They also went down in body size, whereas it took a long time for this to happen in the Jerseys.

Geschwind: Now who was in charge of the beef herds in the department?

Cole: Yes, in the early years, of course, Professor Guilbert was in charge of the beef herd and he was involved with some studies in conjunction with Paul Gregory on growth. They were interested in determining if it were possible to get some linear measurements whereby they could determine what the growth potential of an animal was and they made all kinds of measurements for a long period of time, none of which were entirely successful. But Professor Guilbert did introduce one concept way ahead of his time. He had the concept that in order to have an economical beef animal that it should be a growthy animal that could reach a relatively large

Cole: mature size and therefore he developed the Rover line of breeding in the Hereford herd on the Davis campus which did have this characteristic. It was a fast growing, large animal. At the same time his own herdsman, Alec McDonald, was selecting the exact opposite type when he went into the show-ring. Mr. Alec McDonald was a famous beef herdsman here who had an international reputation as a judge and he invariably placed a small, squatty animal at the top, completely overlooking the fact that this animal was not economical when you considered the rate of gain. As far as the carcass was concerned, yes, these small animals had desirable carcass traits.

As I've mentioned earlier, Professor Guilbert did some very outstanding work on the minimal vitamin A requirements of several classes of livestock. With Dr. Hart and Mr. Kenneth Wagnon, Guilbert did some excellent work on the nutritive value of range plants at different seasons and also showed how deficient ranges could be supplemented. Professor Guilbert had a natural inquiring mind and knew how to organize research even though he received no formal training beyond the M.S. degree. Also, he worked under the handicap that his wife was an invalid for many years following an attack of infantile paralysis.

Geschwind: You previously mentioned the fact that Professor Howell was in charge of the horse herds in the department. Now what was the nature of his research?

Cole: Yes, well, he didn't do a lot of research. As a matter of fact, there was a time when he was doing practically all the teaching of the non-degree students. And I know of the time when he would walk out of one class and into the next one all morning long. And then maybe in the afternoon he'd be in charge of a laboratory. So he, I think, really was treated a little bit roughly as far as teaching assignments were concerned. It's true that he didn't have a strong interest in science and he was not trained as a scientist. He had no training beyond his master's and I don't know what the nature of his master's training was, but obviously he was interested in teaching. He was interested in extension activities. As I mentioned, he did collaborate with me in one or two instances although he didn't do a lot of work actually but he was most cooperative as far as letting me use the horses for the pregnant mare serum gonadotropin studies, which was very important from my point of view.

Geschwind: You mentioned earlier that Dr. Catchpole had gone down to the Kellogg Foundation. Did Dr. Howell have anything to do at all with the famous Arabian horse herd in Pomona?

Cole: Yes, he went down there as the director of the farm for a number of years. I've forgotten exactly, I suppose it must have been

Cole: six or seven years until the University decided to give up this breeding program. They gave up the Kellogg Arabian Horse Farm during World War II. The University decided at that time that they could not afford to keep it and turned it over to the army, I think it was, and then it finally went to San Luis Obispo State College. Professor Howell was in charge of this establishment for a number of years and then when he came back to Davis.

Geschwind: What was he trying to do with the establishment?

Cole: Well . . .

Geschwind: Was it simply to breed purebred Arabians?

Cole: No--well, no he was interested in more than this. He brought Dr. Paul Pearson in there to do nutrition work or at least Pearson was sent down there by Dr. Hart, I presume, to do nutrition work on horses. And they did some work on calcium and phosphorous in the nutrition in the horse down there. Pearson also collaborated with Dr. Catchpole on the half-life of the PMSG study that I mentioned and they were interested in reproduction in horses. Professor Howell himself did not get involved with any research activities down there to my knowledge. But, of course, he had many diverse activities; he was running a horse show every Sunday in addition to keeping the farm going.

Geschwind: A man who was in charge and responsible for the University's sheep flocks was Professor Wilson. What were his major interests?

Cole: No, he wasn't actually in charge of any flocks. He came here as a wool technologist and he did do some studies on the technology of wool.

Geschwind: Who was responsible for the flock then?

Cole: Well, Professor Miller was in charge of the sheep flock and did some applied studies. He studied, for example, the most desirable type of cross-bred lamb. He crossed Rambouillets onto Suffolks and Hampshires and studied the rate of growth of these lambs and their mutton characteristics at the time of slaughter. He also carried on some feeding trials with lambs and this sort of thing. Of course, his death was premature, he developed a tumor of the brain and died before he was sixty, I think.

Geschwind: Now I understand that there was a bit of good-natured rivalry between you and Jim Wilson over a period of years.

Cole: No, I never felt there was any rivalry.

Geschwind: I don't mean that in a way of competition (laughter) but rather in terms of playing practical jokes (laughter).

Cole: Well, the only practical joke that immediately comes to hand is we decided--we both smoked quite heavily in those days. We decided that we would restrict our smoking to three cigarettes a day. And the first thing we knew, well, one or the other of us were fudging one way or another. I finally found some Mexican cigarettes which were about eight inches long. So I came around one day smoking one of these Mexican cigarettes (laughter) and he said, "Well, you son of a gun." He said, "Well, the truth of the matter is I've been fudging a little myself (laughter)." So I think that after about eight months our three cigarettes a day went by the board (laughter) but other than that I can't think of any practical jokes that we played on each other.

However, I don't think that a person as colorful as Jim Wilson was should be passed over so lightly. He was a very important man in the department even though his contributions in research were not great and even though his teaching in later years was minimal. He had a veritable mountain of anecdotes which were sprinkled throughout his lectures in the classroom and before livestock groups. Furthermore he had a good grasp of the knowledge in the field of wool technology. In essence he was a very effective teacher and it is unfortunate that wider use was not made of his capabilities. He was a colorful writer and a frequent contributor to those livestock journals concerned with sheep production.

I don't think that he was greatly interested in research. I recall on one occasion he, with the help of Dr. Paul Gregory, drew up an elaborate scheme of crossbreeding of sheep in which it was contemplated to develop all of the most desirable wool characteristics and at the same time the most desirable mutton traits in this new breed. To be certain that animals of the very best wool characteristics were used he suggested that he be sent to Australia to select some of their fine Merinos. Dr. Hart called a department meeting in which this plan was laid out. Several of us questioned the possibility that such a unique animal could be developed and also questioned whether it was necessary to go to Australia. Well, it turned out that his ticket to Australia had been purchased and he was to leave in a few days. However, it was found that there was an embargo on exporting Merinos from Australia so he had to settle on animals from New Zealand. Anyhow, the animals were purchased and the experiment set up. However, Gregory had many other interests and Jim Wilson did not supervise the breeding program closely enough. As a consequence the wrong rams were used one year. The program did yield the Ph.D. thesis, however, for Daniel Cassard but we are still looking for this ideal sheep that was envisioned.

While in Australia he learned of the elastrator used for castrating and docking lambs. He received the rights to produce

Cole: and market this rubber band and the instrument used in stretching the band for use. As a result, he established a small manufacturing firm in Davis which his son, John, controls.

Jim was very highly involved in community affairs and a month or two before his death he was made the Citizen of the Year for the City of Davis.

Geschwind: The last person I can think of in that group, at least in terms of names with which I'm familiar is Professor Hughes.

Cole: Yes. Professor Hughes, of course, did some early work on barley as the sole feed for swine and the deficiencies that resulted. But this is the sort of an experiment that doesn't get anywhere. It takes an awfully long time to determine all of the deficiencies that are present and try and sort them out one by one. But he went back and got his Ph.D. from the University of Wisconsin after he had been here for ten years or more and when he came back there was a young man in the Department of Poultry, Dr. Thomas Jukes. And Dr. Thomas Jukes was very much concerned with the vitamin requirements of chickens. He, as a matter of fact, discovered pantothenic acid. And in as much as both chickens and swine survive largely on high concentrated diets, he got in touch with Dr. Hughes and stimulated Dr. Hughes to study the vitamin B requirements of swine. Dr. Hughes went on with his work in which Dr. Jukes was in close contact throughout and made some very important contributions on the various vitamin B deficiencies of swine. This was his major, and a very major contribution. It was rather interesting in that it showed a side of Tom Jukes' personality which I don't think other people recognized. He never asked to be a co-author on any of these studies and I'm sure that Elmer would have been delighted had he been a co-author, on any of them. But Elmer was the sole author on most of these studies. Elmer, of course, became chairman of the department when Dr. Hart left to become Dean of the School of Veterinary Medicine in 1948. And he was doing, I thought, a very excellent job. He had, I think, a better view of the importance of basic research than Dr. Hart had. He stimulated interest in basic research and he eliminated partly the concept of the two types of people in the faculty--scientist and production faculty--in the Department of Animal Science. Of course, this had all been going on, as I mentioned some of the replacements that Dr. Hart had made had excellent scientific backgrounds whereas all of the older faculty had at most a master's degree. They were here when Dr. Hart arrived.

Well, Elmer Hughes dropped dead in his office down here. He was having an interview with Dr. Hart and of course, after his death Professor Harold Guilbert was made acting chairman of the department. But he had a heart attack within, I guess, a

Cole: month after he came in and then I was appointed acting chairman and that was in April 1951, or May 1951. One year later I was made department chairman.

Geschwind: And you stayed in that position until 1960.

Cole: Nineteen sixty, yes.

Geschwind: Tell me something, which I've been curious about, before the School of Veterinary Medicine was set up here, with Dr. Hart in the department as a veterinarian, was the department involved in the teaching of something the equivalent of veterinary science along with the normal production courses?

Cole: No, no. All the veterinary science teaching was done by two veterinarians that were here--Hayes and later Dr. Cameron and still later than that Dr. Hinshaw, who was the poultry veterinarian. So, no, we never--other than this first year or two when he was teaching physiology--the laboratory was taught more or less along the lines that it had been taught in many veterinary departments prior to this. That is, it really was anatomy rather than physiology.

DEPARTMENTAL RESEARCH UNDER CHAIRMAN COLE

Geschwind: All right now you came in as chairman, then, in 1951 and you had much to say in terms of directing the future of the department in terms of your recruiting and so on. And also in terms of the inspiration of faculty members--younger faculty members in which they were large in number immediately after the war. Do you want to recount this period?

Cole: I think that you've recounted the essential picture, that is, I did try to bring in--I thought it was extremely important that we brought in people that could be of equal quality as far as training in the given disciplines with those that one finds in medical schools or anywhere else. And I tried to do this in making the appointments. And I felt that--I had a feeling that the research in the Department of Animal Husbandry and later the Department of Animal Science should be of two types. First as we indicated earlier it's necessary to do some basic research and secondly, by the nature of the department, we are supposed to do some applied research.

I felt that it was not necessary that everyone in the department do applied research. Some people can be involved solely with basic research and their contribution would be in helping keep the other people on the right track. Dr. Goss did this in the very early years, of course, very well. He helped out a lot of people, for example, Professor Guilbert, who never would have been able to carry on his vitamin A studies had it not been for Professor Goss' assistance. Even though Goss is not recognized in much of this research, he played a very important role. I still feel the same that by our nature we must be involved with applied research but the same person can be involved in both applied and basic research. In fact, it is desirable that he be involved with both basic and applied studies. Because if after you finish one applied study as, for example, the early studies on PMSG, you immediately turn to another subject, I think one is missing out a bit because a background is very important in building up more basic studies on the biological nature of the hormone. So I think it is important for all scientists to be involved to a certain extent in basic

Cole: research. I think it's also advisable for some to be entirely involved with basic research and their contribution to the department is in their expertise in their particular discipline for the benefit of other members of the department.

And that's the way the department has gone and it is continuing to go, I hope, if funds can be made available.

Geschwind: What do you see as the highlights of the period during which you were chairman in terms of research?

Cole: Well, I think that the highlights were continuation and development of more basic research. Some very important contributions were made in nutrition and applied physiology. For example, it was--we haven't mentioned previously, of course, I had a graduate student, Dr. M. T. Clegg, who studied the influence if diethylstilbestrol (DES) on growth in ruminants. And he made a number of very important contributions both applied and basic in nature, which I think were of significance. He developed, for example, the technique of implanting the diethylstilbestrol in the ear where there was no possibility of contaminating the carcass. He also felt that it was desirable to determine the minimal amounts of DES which would stimulate growth without undesirable side effects. He did a lot of field studies in collaboration with Dr. Floyd Carroll and Mr. Reuben Albaugh and Mr. Horace Strong. They had twenty or thirty field studies with DES in which they determined the minimal amount of hormone which would give an optimum growth response without these side effects under field conditions. The side effects resulting from an overdose were raised tail heads, masculine behavior, overdevelopment of the forequarters and so forth. Of course, he (Dr. Clegg, that is) made some very important contributions on the role of the hypothalamus on reproduction in collaboration with Dr. Ganong. And this is an area which had no immediate application but in the short span of fifteen years important applications have been made.

During my tenure Professor Gregory became very much involved in the study of dwarfism in beef cattle. This had become a serious problem and with my help and the help of Dean Hutchison, we obtained enough animals for him to start testing of some bulls for the dwarf gene. These were animals that were normal in size but had produced dwarfs. The concept was that dwarfism depended upon a single autosomal gene and his immediate concern then was developing a test for this dwarf gene. Breeding to fifteen cows was supposed to have given us a clue. Well, he got off into a lot of philosophical concepts and made some significant contributions but created a great deal (laughter) of disturbance in the meantime. One incident I recall was he had one bull that was given to him by Professor Guilbert, which was small in stature but was not a typical dwarf.

Cole: He developed a concept that this animal was homozygous for the dwarf gene but carried an inhibiting gene which prevented the full expression of dwarfism. And from this time on he spent a large amount of time trying to prove this concept. One thing that he did clearly show was that there was a relationship between many types of dwarfism; for example, the dwarf gene in the Dexter is lethal in the homozygous state, the so-called long-headed dwarf in the Angus breed, and the short-headed Hereford dwarf. By crossing of these Dr. Gregory showed that there was a relationship and he could produce dwarfs by crossing these different types of dwarf animals and showing that they did have a gene in common. But he made many enemies in the meantime because he came to the conclusion that the particular line of breeding in the University herd was a carrier of the dwarf gene. Well, he never did prove this and I think he has no evidence to support it but we had sold many of these animals to breeders throughout the state with the idea they were dwarf free and of course, they were actually advertising that they were dwarf free so when Gregory went out in the field saying that the Rover line of breeding carried the dwarf gene, this created a good deal of havoc from a practical point of view. It also created some problems for me, of course, with the beef industry. But he became rather a--well I presume one might characterize him as somewhat paranoid in his later years which detracted from the acceptance of many important findings which he made. Several articles which he submitted to the _Journal of Heredity_ and the _Journal of Genetics_ on this subject were turned down. He had a feeling that Dr. Jay Lush at Iowa State University, and Dr. H. H. Stonaker at Colorado State University were out to get him and they were preventing these papers from being published. Actually, they did _not_ turn down, or whoever were the reviewers did not turn down his papers carte blanc but merely said that they would only be acceptable after certain revisions. Dr. Gregory refused to make these and so he would send them off to other less prestigous journals to be published--some of them never did see the light of day, I guess, but others were published in publications such as _Growth_, which did not have a very effective editorial review as far as I know. Anyhow, they did publish some of them which had previously not been acceptable to the journals I have mentioned.

I think one of the highlights during my tenure as chairman was the collaboration initiated between our faculty and those of other departments. One example was the collaborative studies of Jim Meyer and Glen Lofgreen with faculty in the Department of Agronomy and the Department of Agricultural Engineering to determine how to obtain the maximum yield of nutrients per acre from irrigated pastures and from alfalfa. The pasturing studies took into account the effect of different management practices such as continous versus intermittent pasturing, for example, on the integrity of the stand. Another product of these studies was the

Cole: determination of the best means of evaluating the nutritive value of alfalfa hay. With Agricultural Engineering, different methods of processing hays such as baling, pelleting and so forth were initiated. Some of these studies were extended later by Bill Garrett and Mag Ronning.

Another important collaboration involved the studies on evaluating meat quality in which faculty from Food Science and Technology and Home Economics participated. Floyd Carroll played an important role in these studies. Prior to this time very little attention had been given to the effect of breed, age and dietary regime on the quality of meat. As a matter of fact at the present time sires from a host of exotic European breeds are being used in crossbreeding in which rate of growth is emphasized and little or no attention is being given to meat quality. Studies in which Wade Rollins, Nick Ittner from the Imperial Valley Experiment Station and Floyd Carroll participated with the aforementioned departments showed that meat from Brahman steers was less tender to that from Herefords when they were raised under identical conditions. Crossing of the two breeds resulted not only in a better rate of gain but in meat quality almost equivalent to that obtained from Herefords.

Other important collaborative studies involved those at the Hopland Field Station in which Don Torell, stationed there, and Bill Weir collaborated with faculty from several departments in studies on how best to manage the range in that area for maximum yield and also to study the competition of deer and sheep in the utilization of the range. Don Torell developed the esophagial fistula for sheep as a means of obtaining an accurate account of what forage was consumed. The forage from the esophagus was passed into a bag attached to the neck of the sheep.

Worthy of mention were the collaborative studies of Nick Ittner and later Bill Garrett at the Imperial Valley Field Station with USDA engineers stationed at Davis to determine the best means of making animals comfortable in hot desert areas. Use of wire rather than wood corral fencing, cooling the drinking water, spraying the animals with a fine mist and limiting the forage in the ration, and shades were some of the practical means of lowering the body temperature of the animals during hot weather.

The joint studies of Tran Clegg with Bill Ganong in the Department of Physiology on the San Francisco campus afforded another important bridge between these institutions and at the same time yielded some important findings concerning the role of the hypothalamus in controlling reproductive activity.

Also during my tenure there was a very close and fruitful collaboration between Max Kleiber and Arthur Black and Jerry

Cole: Kaneko in Veterinary Science. This also applies to the collaboration of Paul Gregory and the anatomists in the School of Veterinary Medicine.

Finally, it was during my tenure that extensive collaboration with Agricultural (now Cooperative) Extension was initiated. The wealth of information on how to most effectively use DES to increase the economy of beef production resulted from the collaboration of Tran Clegg with Horace Strong, Reuben Albaugh and with extension people in the county offices was a classical example as to how Extension and production departments could integrate their activities. Probably no other single activity did more to cement our relations with the California Cattle Feeders than these studies.

During my tenure the extension specialists were moved into the Animal Science Building--at least three of the four specialists. This has brought the specialists in closer contact with the Animal Science faculty.

The important factors in all of these instances is that each collaborator provided an expertise which was necessary in attacking the problem and equally important each collaborator had sufficient interest in the project to assure its success. How important this latter factor is can be illustrated by a collaborative project on the role of calcium and phosphorous on reproduction which was set up shortly after my arrival in Davis. Dr. Donald Bisson in Chemistry was to run the analyses of calcium and phosphorous, Dr. C. S. Mudge in Dairy Industry was to determine the dietary influence on the physical properties of the milk, Professor Regan and Professor Mead were to supervise the care of the animals and collect the required samples of feed, urine, feces and milk. Dr. Goss, as I recall, was to determine blood levels of these minerals; I was to study the reproductive organs and Dr. Hart was to have overall responsibility for the project. On paper, it looked like a great project but unfortunately each of these people had other major responsibilities and most of us did not have a burning interest in the project. So, in spite of all the planning the project was a complete dud.

OBSERVATIONS ON CHANGING DEPARTMENTAL TEACHING PATTERNS

Geschwind: To what extent did the teaching in the department change in terms of expansion of courses and so on during your tenure?

Cole: Yes. Well, there were only production courses of a very superficial nature in the years preceding Dr. Hart. Professor True, who was chairman of the department prior to this time had no particular scientific background and all of the people he brought in had a relatively minimal amount of scientific background to go on. So the courses were rather superficial. The one exception to this was Professor Woll, a Norwegian, who was brought in here with a Ph.D. from the University of Wisconsin. But as Professor True pointed out in a memorial written for Professor Woll, there really were no facilities where Professor Woll could do any research when he got here. They had no laboratory and apparently Professor True didn't think it was necessary to have any laboratories. Why they would bring in a highly trained person as he was under these particular circumstances--it's a little difficult to understand. But Professor Woll wrote about eight different textbooks and therefore when he was at the University of California, his whole--his main activity was writing these texts and keeping them up-to-date. He did teach Feeds and Feeding for at least a year or two but I couldn't find any other evidence that he contributed anything to the department.

Anyhow, when Dr. Hart did come in, he introduced courses in nutrition, in genetics, and in physiology. And I felt that this was extremely important to have these courses in the department. As a matter of fact, later on when it was suggested that a Department of Animal Physiology be developed on the Davis campus, I at first was opposed to this idea because I felt that if departments of genetics and physiology and nutrition were set up separately that this would eventually mean a draining of the personnel out of the Department of Animal Husbandry. And I think that this probably might have been the result excepting that we were in a rapidly expanding era when the number of students was greatly increasing and therefore, we took out these courses one by one. First a Department

Cole: of Genetics was set up. Then eventually it was decided to set up a Department of Animal Physiology and then that meant that Animal Husbandry 110, the conventional course given in Animal Husbandry which I gave for over twenty years and which Dr. Boda taught after I became chairman went over to this department.

But then it did not turn out as I had envisioned because as I said the campus was rapidly expanding and there were enough students available so that advanced courses in physiology and genetics and nutrition could be developed within the Department of Animal Science and thus maintain the contact with the discipline which I think is very important for our faculty members in animal science to have. If this had not been true, if the enrollment had been low and these discipline departments had been developed it might have been the end of the Department of Animal Husbandry or it might have reverted back to the type of department in which teaching was restricted to production courses and research restricted to the type carried out by state universities because it would have been difficult to recruit first class faculty. But as it was, I think it turned out in a very satisfactory way.

Geschwind: About what time or what year did the non-degree certificate disappear from the campus? I know the department was heavily involved in teaching for the A.A. degree.

Cole: Yes, you're referring to the non-degree subjects.

Geschwind: That was a two-year student program.

Cole: I think it ended--I can't--I'd have to peruse the catalogues to be sure of this, but I think it was in the late--well, perhaps it was the time that teaching was suspended during World War II when there was about a two-year period in which there was no teaching on the Davis campus. When the army moved in all teaching facilities were taken over by the army. So I think that was probably the end of it. And they just didn't renew it after that. I'm not certain about that point.

Geschwind: How much contact did you as a professor in the College of Agriculture have with the people who came up from Berkeley to teach the vet courses and letters and science on campus?

Cole: Which courses?

Geschwind: The breadth courses in letters and science in terms of American History and English and so on?

Cole: Well, of course, actually most of those people were brought in on a permanent basis. They had brought in Celeste Wright in English.

Cole: Dr. Vernon Puryear was the first historian on the campus and he very shortly thereafter brought in Dr. James Shideler. And actually there weren't very many people who commuted. One exception that I remember very vividly was Dr. Kenneth Galbraith, who taught the beginning course in economics on this campus for at least one year. As a matter of fact, he occupied the tiny office right next to the women's rest room now occupied by Roy Hull (laughter).

Geschwind: (Laughter) In this building.

Cole: (Laughter) Yes. Perhaps had we realized that Dr. Galbraith would one day become our ambassador to India, more elaborate quarters would have been provided. Also it is rather disquieting that the future professor of economics and a famous author should have been treated so shabbily.

Geschwind: Now in looking back on the department--of the present members of the department, Professors Carroll, Cupps, Heitman, Lofgreen, Meyer, Ralston and Rollins were all appointed by Dr. Hart or before you became involved in administrative affairs.

Cole: Yes.

Geschwind: Would it be correct to say that aside from Professor Boda and Dr. Clegg who are no longer in the department that Professors Bradford and Ronning and Weir were your appointees?

Cole: No, Professor Weir preceded me too as also did Dr. Robert Laben. Dr. Hughes appointed Bob Laben when Dr. N. P. Ralston resigned.

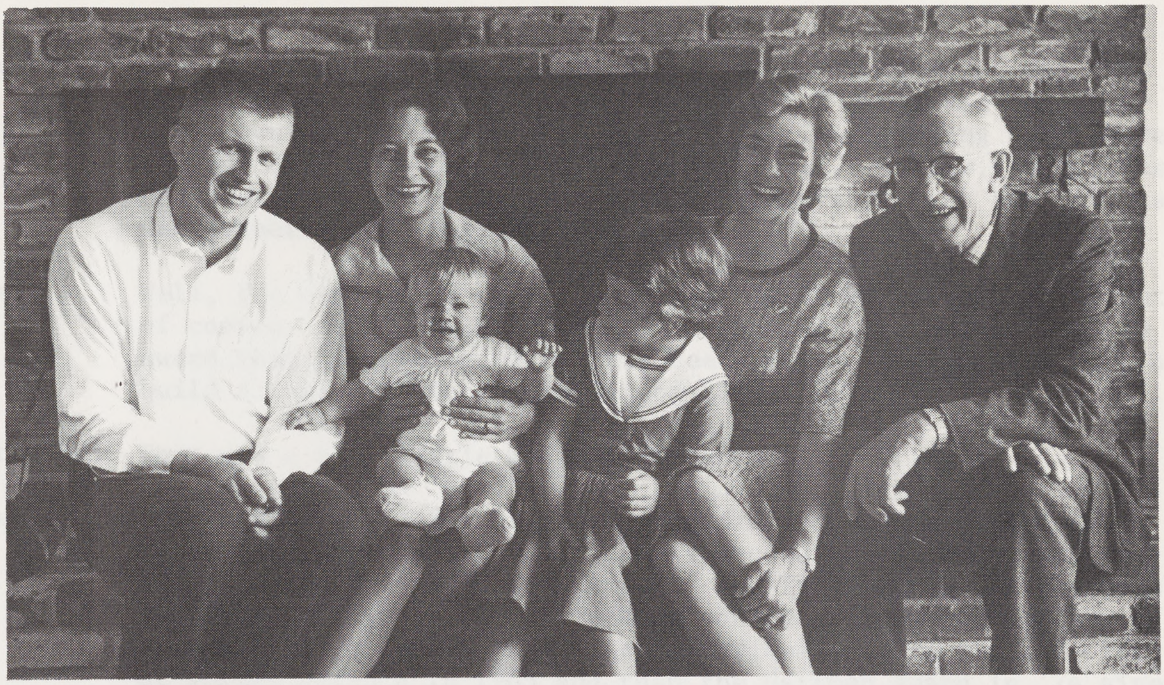
Geschwind: Professor Weir preceded you as well. So it's Bradford, Ronning, Boda, Clegg and myself would be . . .

Cole: And Loy--

Geschwind: And Bob Loy would be the six individuals. And during that period both Bill Garrett and Verne Mendel were graduate students?

Cole: That's right and I also hired Garrett. I brought him in first to go down to the Imperial Valley. But when I sent him down there I promised him that if I had the say-so I would see that he got a position up here when one became available and I was able to complete that as you know.

Geschwind: Well, the last question now really that I will ask you is because you brought up the matter of Clegg's studies on diethylstilbestrol as a growth promotant. You have been carrying out some interesting correspondence on the one hand and also leading a committee--having a committee for the Society of Animal Science which has to do with diethylstilbestrol feeding as supplementation in cattle.



From left to right: Cliff Cole, Audrey Cole and daughter Julie Ann, Nancy, Cynthia and Harold. 1960.

Receiving the Physiology and Endocrinology Award, American Society of Animal Science, 1963.

Geschwind: Now the industry has honored you at times. I know you won the Golden Fleece Award by the wool growers. I don't know how many other awards you've gotten. I see there is something over there on your desk with some soap stone and metal. I know you got the Hartman Award for your studies on reproduction. Can you tell us what other awards you have gotten.

Cole: Well, the only award that I had from the livestock people directly, of course, is the Golden Fleece. I consider it more or less an award that the California Cattle Feeders gave us enough money to build a feed mill to the amount of $165,000.

Geschwind: That was a nice little momento (laughter)!

Cole: (Laughter) But as far as research awards, of course, the first award that I had was that I was the second faculty research lecturer in 1943 on the Davis campus. Dr. Fred Briggs was the first. And then, of course, came the Morrison Award from the Society of Animal Science in 1952-53. The Endocrine Award from the Society of Animal Science in 1963 and then the Hartman Award in '74 given by the American Society for the Study of Reproduction. I also received an honorary LL.D. degree from UCD in 1965.

Geschwind: Now you've been involved in a large number of committees in these various societies, especially Animal Sciences.

Cole: Oh yes.

EMERITUS RESEARCH ACTIVITIES

Geschwind: Now would you like to talk in closing a bit about these most recent ventures with diethystilbestrol which might tie in again with Professor Jukes?

Cole: Yes. Rather strangely Dr. Jukes and I have a common interest. Of course, Dr. Juke's interest is wider than mine. He is interested in the question of the use of feed additives in general. Mine has been specifically with diethylstilbestrol and other estrogenic hormones. Well, as you mentioned, I was made chairman of the subcommittee on hormones of the feed additives committee of the American Society of Animal Science and I was chairman of that committee for a number of years and then asked to be relieved of the chairmanship. But during this time we prepared a paper largely as a result of my efforts on the safety of DES as a growth promotant in cattle. This paper was submitted three times to *Science*; after the first submittal there were certain suggestions that had been made for its improvement. Well, we followed these suggestions and it was submitted again but the second time apparently they had a different set of reviewers so they made a different set of suggestions so we revised it again and it was turned down for the third time. After the third rejection, I wrote to Dr. Abelson, the editor of *Science*, and said that I was very interested in knowing what the comments of the reviewers were no matter how distressing this might be to me. He didn't answer the letter but he sent back the two reviewer's comments and both of them recommended that the article be published. Therefore, Dr. Abelson had taken it upon himself to reject the paper because it was contrary to the policy of *Science*. It is amazing to me that *Science* would take such a biased position. *Science* magazine, both before and after, has written a series of articles in which they have favored the banning of DES as a growth promotant. They had several staff writers writing articles suggesting that it be terminated because of the Herbst finding of adenocarcinoma of the vagina in daughters of women who had taken DES to prevent abortion.

More recently they have been writing articles opposed to the use of the morning-after pill or the five day treatment of twenty-five

Cole: milligrams per day for five days to prevent pregnancy following (I was going to say mating laughter).

Geschwind: Insemination.

Cole: (Laughter) More recently, they have been writing--the staff writers--have been writing articles on the bad side affects of the pill, particularly the sequential pills in which they give estrogen for a certain period of time followed by progesterone for a few days. Apparently this has not involved the initial pill, Enovid, which contained both estrogen and progesterone. But they've shown that eighty-five micrograms of certain estrogens does have some bad side effects such as cystic gall bladder, for example, which may be a precursor to gall stones. So--and also cardiovascular symptoms, of course, have resulted from it.

But anyhow to get back to this article which was turned down for the third time, it was eventually published in Bio Science and has proved to be of great interest . . .

Geschwind: Well, would you summarize your general feelings about this whole area?

Cole: Yes, I am still of the view that diethylstilbestrol can be safely used. That the amount of the increase in the estrogen level as a result of eating liver, for example, would be about one-tenth of one per cent of the level which is normally secreted daily by a human female at the height of secretion. Therefore, when one considers that the average person eats only 1.7 pounds of liver per year, that means that they would be consuming--they would be eating, assuming that they ate one-fourth pound at each meal, it about seven times per year. I can't possibly visualize any serious affects because we all know to get a carcinogenic response or a cocarcinogenic response, you must give the estrogen constantly over a period of days, weeks or months. It doesn't make any difference if you're giving natural estrogens or a synthetic estrogen. All the estrogens will increase the incidence of tumors but you must give them over a considerable period of time. The only exception to this that I know of is in the neonatal mouse and probably during fetal life at which stage one can get a precancerous state by a single injection. Dr. Nandi and his associates in Berkeley have worked on the neonatal mouse. But I've considered that there's no possibility of a--increasing cancer with any normal amount of hormone. Of course, one person in the Nutrition Department of NIH, Dr. Leo Friedman, challenged me one time and said you can't make too much of this factor that you're just eating liver now and then. Remember there may be one person in the United States that eats liver every day of the year. But I maintained in reply that if a person ate liver every day of the year, he'd be in serious trouble as far

Cole: as gout was concerned and with other metabolic diseases which were much more serious than the possibility of cancer he might be subjected to. So I think that it's completely safe. I realize that some cattle feeders have violated the regulations. This law as with any law, is subject to certain regulations on how much you can give and if you exceeded these regulations, you may be in trouble but this is true with all regulations. They can be violated.

Geschwind: Well, in terms of maximum production of food, what does this mean to the beef industry to be allowed to utilize diethylstilbestrol?

Cole: Well, it means for example that there are several million acres that are now--in pasture that would have to be devoted to getting this animal to prime state that can be saved by using this hormone. By the use of the hormone one obtains a 10 to 15 per cent increase in efficiency. Now, of course, they are doing this at the present time--they are using other estrogens but we--there is no reason to suspect that they are any safer than diethylstilbestrol. Estradiol, for example, is being used both with and without progesterone and Ralgro, zeranol, is used and the only reason they are still being approved is that Ralgro has not been used in humans and massive doses of estradiol have never been given to humans. We have a problem with estradiol--it's a naturally occurring hormone and this is apparently the reason that it is still being allowed--in other words one cannot distinguish in the liver between hormone secreted and hormone administered.

Since you have brought up my work on this hormone subcommittee, I would like to refer back to two other appointments. In about 1943 I was appointed chairman of a subcommittee, Committee on Animal Health of the National Research Council to prepare "A Review of Bloat in Ruminants." Other members of this committee were Max Kleiber, C. F. Hufman of Michigan State University, A. F. Schalk of Ohio State University and T. M. Olson of South Dakota State College. Dr. Kleiber and I were on the committee because we had made some basic studies on the amount and type of gases produced in the rumen on bloat-provoking and non-bloat-provoking rations and I, with Tom Mead and Bill Regan had shown how bloat could be produced at will on alfalfa pasture. This was a comprehensive report in which 220 research papers were cited. It was published in the Journal of Animal Science 4, 183-236 (1945).

This subcommittee with some changes in personnel, published a second review in 1956. This was publication 388 of the National Academy of Sciences - National Research Council. Dr. Hungate was a member of this subcommittee so 50 per cent of the membership were recruited from Davis.

Dr. Jim Boda and I published a third review upon the request of the Dairy Science Association. This review appeared in the Journal of Dairy Science 43, 1585-1614 (1960).

Geschwind: Well, we've come to the end of a long session. Are there any last words you have in looking back over these almost fifty years at Davis?

Cole: Well, I do see some problems ahead as concerns the University and of course the Department of Animal Science. It seems to me that administration on our campus is becoming unwieldly. A few years ago someone made a similar remark to Chancellor Jim Meyer. He replied that as a matter of fact that the administrative cost per student is going down. Of course, there are many ways of calculating administrative costs. I can recall when Bill Weir was Dean of Students and his salary was totally allocated against Animal Science. I mentioned to Provost Freeborn that there should be some adjustment. In essence he replied that when Bill spent more than 50 per cent of his time in the Dean's office this would be considered. More recently there have been instances in which one-half of the salary was charged against Animal Science when the individual was doing much more than 50 per cent administrative work. The proliferation of administration is reflected back onto the departments in many ways. For example, I can recall when I was chairman my secretary did all of my typing, kept the books, took care of the payroll, handled the assistantship and scholarships of the graduate students and had a little time left over to type manuscripts. Now, we have a full-time business agent with a full-time secretary, a person to take care of counseling of students and another person to do typing relating to administration matters. I recognize that we have six times as many undergraduate and graduate students now as compared to the period I have mentioned. There are also more faculty and support staff. I think we can assume that everyone is just as efficient now as then but the burden put on us by administration is increased. This is reflected back on the department in other ways. Faculty have more committee assignments than previously and many systems have become more cumbersome. Take for example, recruiting for new faculty members. First, we must advertise the position in Science or some similar outlet and state that we are a fair employment organization. Then when applications are received an Animal Science faculty committee screens the candidates and decide on which two or three will be brought to the campus for an interview. Then they must be brought here at our expense, they give a seminar, and are interviewed by all the faculty members. Then there is a vote by the faculty as to whether one of these candidates meets our standards. This is a costly and time-consuming process. Compare this with the system in vogue when I was chairman. First, the chairman would discuss at an open faculty meeting the type of training desired for an open position. Then the chairman would contact prestigious faculty at other institutions as to whether they had any graduate students which met our qualifications. It was imperative that only faculty requiring a rigorous training be contacted. Or in a few instances I knew of outstanding individuals

Cole: because of their published research and contacted the candidate directly. In most instances I visited the campus and talked with both the candidate and his professor. Of course, I selected two members which had been my graduate students because the training in Animal Physiology at UCD is a rigorous one, because both men were outstanding students and had demonstrated unusual capacity to do research and finally because both of them had a B.S. in Animal Science and were familiar with domestic animals. I don't recall that the faculty was brought into the final decision making process. At least six of the seven men I recruited proved to be outstanding and I'm confident compare very well with any selected by the present procedure. Admittedly, the present system has inbuilt checks and balances but is it overall worth the great effort involved?

Another increased burden on both administration and faculty is the large number of majors in the College of Agriculture. I recently had occasion to check on the number and by my count there were thirty-two plus an individual major which could greatly escalate the total number. It seems to me that with such a wide selection and with the flexibility of each major there is no need for the "individual major" and possibly there may be excessive overlapping in the other majors.

Another nagging problem is the lowering of standards as indicated by the high grade point averages. I think a major factor in the prestige of the UCD College of Agriculture has been the high standards set up for both undergraduate and graduate training. Apparently this lowering of standards is nation-wide and dates back to the tensions developed during the Vietnam war.

As far as the Department of Animal Science is concerned it's extremely important from the standpoint of both teaching and research that we hire people brought in highly trained in a given discipline and in only that way can we maintain and hold an equal status with people in medicine and other fields--biological fields. I would think this philosophy would apply to other departments as well.

It may be helpful to say something about my problems relating to membership and participation in scientific societies. In my early years, the policy of the University was to pay expenses to one scientific meeting every other year providing the faculty member presented a paper. Because my early research was on reproductive physiology and because most researchers in this field were anatomists I decided to join this organization. As my research became oriented toward endocrinology, I decided to join the Endocrine Society originally called the Society for the Study of Internal Secretions. Then I became interested in growth and a

Cole: fairly large number of papers was published in the American Journal of Physiology so I joined this society. In the late '30s and early '40s I became interested in more applied fields such as bloat and parturient paresis and consequently joined the American Society of Animal Science and the American Dairy Science Association. Very early in my career I joined the Society for Experimental Biology and Medicine because this organization had a local chapter, which met several times yearly at Berkeley, Palo Alto, San Francisco and Davis and this provided an opportunity to become acquainted with biologists at the several institutions in Northern California. In the late '40s or '50s, I became interested in wider aspects of educational and research activities and thus joined the American Association for the Advancement of Science. At one time or another I attended meetings and presented papers at each of these various organizations but I did not regularly attend the meetings of any of them. Consequently I did not participate in very many committee activities of any of them. I was local chairman for the fall meeting of the Physiological Society held in Davis about 1970. Also I was associate editor of the Journal of Animal Science, the official journal of the American Society of Animal Science for a few years and as I stated earlier I was the first editor of Biology of Reproduction, the official journal of the Society for the Study of Reproduction. On two occasions, I was nominated for the vice presidency of the American Society of Animal Science but in both instances I was defeated by the retiring editor of the Journal of Animal Science. About 1955 I obtained an NIH grant with travel allowances but even then I did not attend the meetings of any society regularly. I think if I had a chance to do it over again I would have restricted my membership to fewer societies--perhaps one discipline society and one society oriented toward the overall objectives of the Department of Animal Science.

Another problem confronting the Department of Animal Science is the extent of participation in extension activities. On the one hand it should not be so extensive as to interfere with the research and teaching responsibilities and on the other hand it must be sufficient to assure the livestock industry that we have a sincere concern about their problems and are doing something about it. To a certain extent the Cooperative Extension Service can act as an intermediary but I think the faculty must always have some direct contact with industry.

Geschwind: What do you see Animal Science headed for in the future?

Cole: I would visualize--you mean as far as the types of research?

Geschwind: Yes, types of research. How do you see it changing? The department is changing, we are bringing in behaviorists, we now have system analyst people in the department and so on.

Cole: I don't--frankly I don't know just what effect system analysis and this sort of training will have but I'm confident it will increase the sophistication of our research. I believe that behavior is a logical extension of our activities, even though we have problems, I think, of training people with the same degree of basic background as they have in physiology for example or in nutrition. I think, in other words they have some way to go in the development of behavior studies to make them as scientific as we do in these other very well-defined disciplines. I think in general that the advances that are going to be made in these fields with more sophisticated techniques will be comparable to the well-established disciplines. We see the writing on the wall, for example, where we may go eventually in using hormones to control reproductive processes as a result of the studies on hormone levels in domestic animals that are being undertaken as a result of and the development of radioimmunoassay. I'm sure we're going to use more sophisticated techniques in all areas of animal science than have been used in the past.

Geschwind: You just mentioned sophistication. Was there ever a time in this department's history that you know of where there was a push in order to convert it into a department which was more interested in sausage stuffing than in terms of more scientific aspects and production aspects. There are some animal science departments which are still doing quasi research.

Cole: No, I don't think so. I think that Dr. Hart should receive credit for steering in the right direction in this respect. Although, I wouldn't object to it going in the direction of the use of science for the improvement of any animal product, although I think this is more logically a role of food science or let's say of textiles or perhaps the collaboration of Animal Science with these departments.

Geschwind: Harold, thank you very much.

Cole: Well, it's been my pleasure, thank you.

APPENDIX A

H. H. Cole List of Publications - Complete Papers

Cole, H. H. & G. H. Hart. 1930. The potency of blood serum of mares in progressive stages of pregnancy in effecting the sexual maturity of the immature rat. Am. J. Physiol. 94:57-68.

Cole, H. H. & G. H. Hart. 1930. Sex hormones in the blood serum of mares II. The sera of mares from the 222nd day of pregnancy to the first heat period post-partum. Am. J. Physiol. 94:597-603.

Cole, H. H. 1930. A study of the mucosa of the genital tract of the cow, the special reference to the cyclic changes. Am. J. Anat. 46:261-301.

Cole, H. H., C. E. Howell & G. H. Hart. 1931. The changes occurring in the ovary of the mare during pregnancy. Anat. Rec. 49:199-209.

Cole, H. H. & R. F. Miller. 1931. The vaginal smear of the ewe. Proc. Soc. Exp. Biol. Med. 28:841-3.

Goss, H. & H. H. Cole. 1931. Sex hormones in the blood serum of mares. Some chemical properties of the ovary-stimulating factor. Endoc. 15:214-224.

Evans, H. M. & H. H. Cole. 1931. An introduction to the study of the oestrous cycle in the dog. Mem. Univ. Cal. 9(2):65-118.

Cole, H. H., H. R. Guilbert & H. Goss. 1932. Further considerations of the properties of the gonad-stimulating principle of mare serum. Am. J. Physiol. 102:227-40.

Cole, H. H. & R. F. Miller. 1933. Artificial induction of ovulation and oestrum in the ewe during anoestrum. Am. J. Physiol. 104:165-71.

Hart, G. H. & H. H. Cole. 1932. A practical method for the diagnosis of pregnancy in the mare. J. Am. Vet. Med. Assoc. 80:604-14.

Cole, H. H., G. H. Hart, W. R. Lyons & H. R. Catchpole. 1933. The development and hormonal content of fetal horse gonads. Anat. Rec. 56:275-93.

Cole, H. H. 1933. Ovogenesis in the ewe and cow. Proc. Soc. Exp. Biol. Med. 31: 241-3.

Hart, G. H. & H. H. Cole. 1933. The gonad-stimulating hormone of mare serum. Rec. of Proc. Ann. Meeting, 1933. Am. Soc. An. Prod.:262-8.

Catchpole, H. R. & H. H. Cole. 1934. The source and distribution of oestrin in the pregnant mare. Anat. Rec. 59:335-47.

Hart, G. H. & H. H. Cole. 1934. The source of oestrin in the pregnant mare. Am. J. Physiol. 109:320-3.

Cole, H. H. & G. H. Hart. 1934. Concerning gonadotropic substances in mare serum. Proc. Soc. Exp. Biol. Med. 32:370-3.

Cole, H. H. & F. J. Saunders. 1935. The concentration of gonad-stimulating hormone in the blood serum and of oestrin in the urine throughout pregnancy in the mare. Endoc. 19:199-208.

Catchpole, H. R., H. H. Cole & P. B. Pearson. 1935. Studies on the rate of disappearance and fate of mare gonadotropic hormone following intravenous injection. Am. J. Physiol. 112:21-6.

Saunders, F. J. & H. H. Cole. 1935. Two gonadotropic substances in mare serum. Proc. Soc. Exp. Biol. Med. 32:1476-8.

Cole, H. H. & R. F. Miller. 1935. Changes in the reproductive organs of the ewe with some data bearing on their control. Am. J. Anat. 57:39-97.

Saunders, F. J. & H. H. Cole. 1936. Age and the qualitative ovarian response of the immature rat to mare gonadotropic hormone. Proc. Soc. Exp. Biol. Med. 33:504-5.

Saunders, F. J. & H. H. Cole. 1936. Means of augmenting the ovarian response to gonadotropic substances. Proc. Soc. Exp. Biol. Med. 33:505-8.

Cole, H. H. 1936. On the biological properties of mare gonadotropic hormone. Am. J. Anat. 59:299-331.

Hart, G. H. & H. H. Cole. 1936. Reproductive hormone therapy in domestic animals. J. Am. Vet. Med. Assoc. 88:12-22.

Cole, H. H. 1937. Hormones concerned with reproduction, their use as therapeutic agents and in pregnancy diagnosis. Univ. Penn. Vet. Ext. Bul. #66:1-17.

Cole, H. H. 1937. Superfecundity in rats treated with mare gonadotropic hormone. Am. J. Physiol. 119:704-12.

Cole, H. H. 1938. High gonadotropic hormone concentration in pregnant ponies. Proc. Soc. Exp. Biol. Med. 38:193-4.

Cole, H. H. & G. H. Hart. 1938. The effect of pregnancy and lactation on growth in the rat. Am. J. Physiol. 123:589-97.

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Cole, H. H. & J. Erway. 1941. 48-Hour Assay test for equine gonadotropin with results expressed in international units. Endocrinology 29:514-9.

Cole, H. H., S. W. Mead & M. Kleiber. 1942. Bloat in cattle. Calif. Ag. Exp. Sta. Bul. 662.

Cole, H. H. & G. H. Hart. 1942. Diagnosis of pregnancy in the mare by hormonal means. J.A.V.M.A. 101:124-8.

Miller, R. F., G. H. Hart & H. H. Cole. 1942. Fertility in sheep as affected by nutrition during the breeding season and pregnancy. Calif. Ag. Exp. Sta. Bul. 672.

Mead, S. W., J. Britton & H. H. Cole. 1943. Cause, prevention, and treatment of bloat. Calif. Ag. Exp. Sta. leaflet.

Cole, H. H. & H. Goss. 1943. The source of equine gonadotropin. In Essays in Biology in Honor of H. M. Evans, 107-119. Univ. Calif. Press., Berkeley, Ca.

Kleiber, M., H. H. Cole & S. W. Mead. 1943. Bloat in cattle and composition of rumen gases. J. Dairy Sci. 16:929-33.

Cole, H. H., S. W. Mead & W. M. Regan. 1943. Production and prevention of bloat in cattle on alfalfa pasture. J. Anim. Sci. 2:285-94.

Cole, H. H., R. S. Amadon, R. W. Dougherty, D. Espe, C. G. Huffman, T. M. Olson & A. F. Schalk. 1943. Recommendations for prevention of bloat in cattle and sheep. J.A.V.M.A. 53:294-6.

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Cole, H. H., C. F. Huffman, M. Kleiber, E. M. Olson & A. F. Schalk. 1945. A review of bloat in ruminants. J. Anim. Sci. 4:183-236.

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* * * *

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